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THE SOLAR ROTATION IN 1911

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1. The determination of the rotation of the sun by the Doppler displacement of the spectral lines at the limb was early placed on the program of work of the Dominion Observatory; was indeed one of the investigations planned for the equipment for solar research whose nucleus was the 20-inch coelostat used in the Canadian Eclipse Expedition to Labrador in 1905. But actual observations have been delayed by various causes, the principal ones being delay in the construction of a shelter for the coelostat telescope[1] and the difficulty of securing a suitable plane grating for the spectrograph,[2] the one now in use being the fourth that has been tried for the purpose. Consequently it was not until the spring of 1910 that any suitable plates were obtained and even these were of a somewhat experimental and tentative character.

2. The whole plan of investigation was placed upon a much more definite basis at the Mt. Wilson meeting of the International Union for Co-operation in Solar Research in 1910. The regions of spectrum to be investigated were allotted to the different members of the Rotation Committee, a general region to be observed by all was selected (center at λ 4250), and the various questions to be

[1] *Report of Chief Astronomer*, 1908, p. 67; 1909, p. 149; 1910, p. 68; also *Trans. Roy. Soc. Can.*, 1911, Sec. III, p. 113.

[2] *Ibid.*

determined were laid down. It may be useful to summarize here the principal points.

A. The region to be observed at the Dominion Observatory is in the yellow green, λ 5500–λ 5700.

B. The general region to be observed by all is from λ 4220 to λ 4280 in the violet.

C. The latitudes to be observed in the special region are 0°, 15°, 30°, 45°, 60°, 75°, and if possible 80° and 85°. The latitudes to be observed in the general region are 0°, 30°, and 60°.

D. Fifteen or twenty lines are to be measured in the special regions, these to be selected to include as many elements as possible, especially those of high or low atomic weight; about ten lines, selected by the secretary of the committee after consultation, are to be measured in the general region.

3. The principal objects of a study of the sun's rotation by the spectroscopic method are:

a) The accurate determination of the velocity of rotation at various latitudes and the derivation of a formula representing the variation of velocity with latitude.

b) A definite conclusion in regard to the existence of variations in the rate of rotation.

c) The investigation of the rate of rotation, as shown by the lines of different elements and of the arc and enhanced lines of the same element, to determine whether either the absolute rate of rotation or the law of variation with latitude differs for different elements.

d) The detection of possible systematic proper motions or drifts in the sun's reversing layer.

4. Although the instrumental equipment used in this investigation has already been described,[1] it is desirable for the sake of completeness to outline briefly the salient features.

The coelostat telescope has a coelostat mirror of 20 inches aperture, a secondary plane mirror of 20 inches aperture, and a concave mirror of 18 inches aperture and 80 feet focus. It is situated directly north of the west wing of the observatory and the beam of sunlight

[1] *Report of Chief Astronomer*, 1909, pp. 207, 251; also *Trans. Roy. Soc. Can.*, 1911, Sec. III, p. 11.

which is reflected south from the coelostat mirror and north from the secondary mirror is then reflected due south, with an angle of depression of 3°5, passing below the secondary mirror and forming an image of the sun about 227 mm in diameter just outside of the observatory basement. The coelostat is covered by a louvred house rolling back on a track and the path of the beam is inclosed by a louvred shelter leading by a short tunnel into the basement. The definition of the image is usually good, better than that given by the equatorial telescope, and does not seem to be injuriously affected by the passage through the tunnel.

The spectrograph, which is of the Littrow form, has a combined collimator and camera objective of 6 inches aperture and 23 feet focus. The grating now in use, a 6-inch plane, was ruled by Anderson at Johns Hopkins; it has a ruled surface about $3\frac{7}{8}\times 5$ inches, 15,000 lines to the inch. Owing to astigmatism only about 2 inches of the length of the ruling is used and it is limited to 3 inches in width to insure a sufficient margin of safety in covering it uniformly by the 5-inch diameter circle of illumination. The spectrograph is of a trussed box form which can be rotated about the optical axis and set to any desired position angle, allowing the reflecting device, used for bringing the light from the limbs of the sun, on the slit, to be set to any required latitude. This device consists of two right-angled prisms, movable radially, which reflect the light from opposite limbs inwardly, and two other right-angled prisms over the slit which further reflect it through the slit. One of the latter is ground to a point, the sides subtending an angle of 30°, while a notch of the same shape and size is ground in the other. This arrangement of the prisms produces on the plate a spectrum from the east limb about 0.9 mm wide situated midway between two spectra from the west limb of the same width, the separation between adjacent spectra being about 0.3 mm. This arrangement is preferable to that in which only two spectra are used, one from the east and one from the west limb, as the correct measurement of the displacement does not depend upon the exact orientation of the measuring wire. All of these prisms are provided with adjusting screws to enable the beams of light coming from the opposite limbs to be exactly collimated, thus insuring uniform and similar

illumination of the grating surface. In front of the prisms is an adjustable guide plate, with small adjustable windows for the admission of the light, which serves to protect the prisms and is devised to enable the desired latitudes and positions on the sun's disk to be easily obtained.

5. A large amount of experimental work to determine the best instrumental and optical conditions, the precautions necessary for accurate observations, the most suitable plates and developers for the two regions of the spectrum to be observed, was carried through in 1909 and 1910 and some preliminary rotation plates were made in 1910, a few of which were measured.[1] After the Solar Union meeting in 1910 and in accordance with the plan outlined in Sections 2 and 3, three series of plates were made in 1911, two in the special region at λ 5600 and one in the general region at λ 4250. The diameter of the solar image averaged about 227 mm; in the first series at λ 5600 the distance from the limb varied from 3.0 to 4.5 mm; in the second series also at λ 5600 it was nearly 10 mm; and in the third series at λ 4250 it was about 6.5 mm. The distance was varied in order to see if any difference in the rotational value was obtained and if much change in the definition occurred as the distance from the limb was increased. As will be seen later, the difference, if any, is slight both in the velocity and in the definition. Owing to the considerably larger corrections required to reduce the measured to the actual values of the rotation as the distance from the limb increases, it is not deemed desirable to make the spectra, in future, from points at a greater distance than 5 mm from the limb.

PRECAUTIONS

6. In all these plates particular care was taken to guard against every known cause of instrumental and other error tending to introduce spurious displacements of the lines, and the experience of one of the writers in stellar radial velocity determinations was of great value in this similar work. Temperature changes and flexure, the chief difficulties in stellar spectroscopy, are not, however, of much moment here, for, owing to the short and simultaneous expo-

[1] *Trans. Roy. Soc. Can.*, 1911, Sec. III, p. 120; and *Report of Chief Astronomer*, 1910–11.

sures on opposite limbs, temperature changes will have no appreciable effect and there can be no flexure when the spectrograph is stationary during the exposure. It may not be amiss to give here the four essential precautions for accurate observations, which have been carefully followed:

a) The emulsion on the photographic plate must be exactly in the focus of the spectrum.

b) The illumination of the grating from the opposite limbs of the sun must be similar and uniform.

c) The solar definition must be good, the image steady, and the sky free from haze.

d) Care must be taken that the reflecting prisms receive light from the desired latitudes.

7. Precautions *a* and *b*, conditions inside the spectrograph, to which may be added the avoidance of undue heating of the slit jaws, are very necessary to prevent systematic displacements of the lines as a whole introducing corresponding errors in the velocity values. If either *a* or *b* is exactly fulfilled, an approximate realization of the other should be sufficient, but, as it is practically impossible either to get or keep the plate at the exact focus or to have absolutely equal and uniform illumination of the lens and grating from the opposite limbs, the only safe procedure is to fulfil both conditions as closely as possible. Consequently the plate focus was determined frequently both by the definition test and, as a check, by the Hartmann method of extra-focal exposures. It was found that the field both in the λ 5600 and in the λ 4250 region was curved, concave to the lens, about 2.5 mm longer at the center than at the ends of a plate 30 cm long and inclined about 1°, in opposite directions for the two regions, to the normal to the axis. The illumination of lens and grating was tested before and after each plate, which consisted usually of seven spectra, one of each of the six latitudes from 0° to 75° and one of the pole. This was done by opening the slit wide enough to allow a visible image of the illuminated concave mirror to be projected on the diaphragmed front surface of the collimating objectives. If this image was not central for both systems of prisms it was easily made so by the adjusting screws provided. It was found frequently that a slight

change in position of the overlapping images occurred during the time the seven exposures were made but never sufficient (since the image is considerably larger than the used portion of the grating) to prevent uniform illumination. This change of adjustment of the prisms must be due to heating produced by the sun's rays, and, to minimize this effect, the heating of the slit jaws, and the distortion of the coelostat, secondary, and concave mirrors, the coelostat mirror, and consequently the whole system, is kept shaded by a blind except during the actual exposures, which occupy from 30 to 60 seconds each.

8. Precautions *c* and *d*, conditions external to the spectrograph, were always carefully looked after. The solar definition during the summer months, on the clear and bright days which only were employed, is usually fairly good and, as undue heating of the mirrors was prevented by keeping them shaded for suitable intervals between the exposures, the definition did not much deteriorate. It is essential that there be fair definition to insure that the light reaching the slit may be confined to a small region around the desired portion on the sun's disk. Great care was taken in the relative adjustment of guide plate and prisms so that when the image was kept central and the spectrograph rotated to the desired and previously calculated position angle from the E.-W. line (determined by the drift of the solar image when the coelostat clock was stopped) the positions of the points on the disk from which the light was taken were accurately known. This is rendered much easier and more certain by the large size of the image (about 227 mm), and consequently it is improbable that any errors can have arisen either in this regard or due to poor definition. The only effect of the latter would be to introduce a small amount of light at slightly higher and lower latitudes or at greater and less distances from the limb and the effects thereby produced would practically compensate one another. The necessity of observing only when the sky is free from haze will be evident when it is realized that the effect of the superposed sky spectrum, which is a blend of the spectrum from the whole disk of the sun, is to diminish the displacement and give too low a value of the velocity. DeLury made some experiments on this effect and found a measurable influence

on the equatorial displacement only when the ratio of intensity of sky spectrum to limb spectrum reached about 1:20. As on a clear day this ratio is 1:100 or less, it is evident that no error can thereby be introduced.

OBSERVATIONAL DATA

9. The plates were made by the authors jointly, since the making of the focus and illumination tests and the guiding of the sun's image carefully can be much more easily and satisfactorily done by two than one. As stated above, in the λ 5600 region, rotation spectra of each of the six latitudes to be observed, 0°, 15°, 30°, 45°, 60°, 75°, with one of the pole, 90°, for check purposes, were made on each plate, but in the higher latitudes 80° and 85° three of each with one of the pole were made on each plate. In the λ 4250 region two spectra each of the latitudes 0°, 30°, 60°, and one at 90° were made on each plate. If any of the plates showed a greater displacement in the spectrum at the pole than about 0.03 km, they were rejected for fear that some instrumental displacement had occurred and that possibly the other latitudes were affected.

10. The plates of Series I and III were measured by Plaskett on a Repsold measuring engine with an eyepiece micrometer, while those of Series II were measured by DeLury on a Toepfer measuring machine with 300 mm screw. The lines measured in Series I and III at λ 5600 and in Series II at λ 4250 are given with intensities, velocity constants, etc., in the following tables. Four settings are made on a line in the center strip and two each on the outside

TABLE I

LINES IN λ 5600 REGION

No.	Wave-Length	Element	Intensity	Velocity Constant	No.	Wave-Length	Element	Intensity	Velocity Constant
1...	5506.095	*Mn*	1	19.336	11..	5598.524	*Fe*	1	18.801
2...	5514.563	*Ti*	2	19.289	12..	5601.505	*Ca*	3	18.788
3...	5514.753	*Ti*	2	19.287	13..	5624.769	*Fe*	3	18.653
4...	5528.641	*Mg*	8	19.207	14..	5638.488	*Fe*	3	18.575
5...	5544.157	*Fe*	2	19.118	15..	5658.097	*Y*	2	18.461
6...	5560.434	*Fe*	2	19.024	16..	5682.869	*Na*	5	18.320
7...	5562.933	*Fe*	2	19.010	17..	5684.710	*Si*	3	18.309
8...	5578.946	*Ni*	1	18.919	18..	5686.757	*Fe*	3	18.297
9...	5582.198	*Ca*	4	18.899	19..	5688.436	*Na*	6	18.288
10...	5590.343	*Ca*	3	18.852					

strips and, after measurement of all the lines, the plate is reversed on the machine and remeasured. This diminishes the danger of systematic errors and also, as the lines are viewed in the opposite direction in the two cases and the number of settings doubled, the accidental errors.

TABLE II

LINES IN λ 4250 REGION

No.	Wave-Length	Element	Intensity	Velocity Constant	No.	Wave-Length	Element	Intensity	Velocity Constant
1....	4196.699	*La*	2	26.906	9..	4257.815	*Mn*	2	26.400
2....	4197.257	*C*	2	26.902	10..	4258.477	*Fe*	2	26.394
3....	4216.136	*C*	1	26.745	11..	4266.081	*Mn*	2	26.331
4....	4220.509	*Fe*	3	26.710	12..	4268.915	*Fe*	2	26.296
5....	4225.619	*Fe*	3	26.666	13..	4276.836	*Zr*	2	26.243
6....	4232.887	*Fe*	2	26.606	14..	4290.377	*Ti*	2	26.133
7....	4241.285	*Fe-Zr*	2	26.502	15..	4291.630	*Fe*	2	26.122
8....	4246.996	*Sc*	5	26.490					

The lines in the yellow-green region were selected to include as many elements as possible among the limited number of measurable lines in the region. Some, such as the lines of *Mn*, *Ti*, *Si*, are not of very good quality for measurement but were included in order to give evidence in regard to question *c*, Section 3, above. In the violet region from No. 4 to No. 13, inclusive, are the ten lines selected to be measured by all the observatories co-operating in this work and the other five are lines which Adams and Lasby[1] found gave systematically higher or lower values of the rotation than the general reversing layer. The column "Velocity Constant" gives the half-value of the multiplier required to reduce the millimeter displacement to kilometers per second and will evidently give the observed velocity of the sun's limb. These multipliers are readily determined, in the well-known way, when the linear dispersion at the region is known. As the grating gives practically a normal spectrum over the narrow limits used, it is sufficient to determine this dispersion, which is about 0.70 Å. per millimeter at λ 5600 and 0.75 Å. at λ 4250, for five or six lines over the region used. When these values and the multipliers are plotted on cross-

[1] Adams and Lasby, *An Investigation of the Rotation Period of the Sun by Spectroscopic Methods*, p. 119.

section paper they are found to lie within the errors of observation on a straight line, and the constants for all the lines measured can be at once read off.

REDUCTION OF MEASURES

11. The observed or measured velocities are the radial components of the actual velocities at certain points on the sun's disk whose latitudes can be readily computed, and it is hence necessary to know the angle of inclination between the radius vector and the direction of motion at the point in order to apply the necessary corrections, the further correction for the motion of the earth in its orbit being made to obtain the sidereal rate. In the early observations, by Dunér and Halm, of the rotation of the sun by the spectroscopic method, the measurements were made at the limb and the computations and corrections were straightforward. When, however, as in Adams' work and our own, the observed points are some distance within the limb, the matter is not quite so simple. Adams' method of reduction depends upon projecting the observed points radially to the limb and obtaining the corrections by Dunér's methods and tables, but this assumes the rotation of the sun to be that of a solid body, which is of course not the case. A further correction is therefore necessary for the difference in angular velocity at the observed and computed points. Nearly all of Adams' plates were made with the observed points close to the limb and this final correction is in the majority of cases inappreciable and only reaches in a few plates, around latitudes 45° and 60°, 0.01 km per second. Nevertheless, as it is always in the same direction, it should be applied. This is especially the case in our own observations where the distance from the sun's limb is frequently much greater and where the value of the correction may reach 0.03 km per second. Two methods have been followed here in reducing the observed to the actual velocity. The first method consists in applying a correction to Adams' method for the change in angular velocity, thus obtaining the sidereal rate at the radially projected point on the limb. The second method determines the correction to be applied to reduce the measured to the sidereal velocity at the actually observed points on the disk. This entails

the determination of the heliographic latitude and longitude and of the angles between the direction of motion and the radius vector at the observed points, the division of each measured velocity into two parts proportional to the angular velocities at the two observed points, and the sidereal corrections. By these two correction methods, three reduced values (at three different latitudes) of each measured velocity displacement are obtained, but of course this does not increase the information obtained from the observations. A comparison of the residuals from the two correction methods (Table VII) shows that so far as accuracy is concerned it is immaterial which is employed.

SUMMARY OF MEASURES

12. It is impossible within the limits of this paper to give the separate measures for each spectrum or even the measured and reduced velocities for each separately, so only a summary of the mean values at each observed latitude is given in the following table. In Series I these mean values are obtained from the measures of 19 spectra at the latitudes 0° to 75° inclusive and 18 spectra at the latitudes 80° and 85°. These were observed on various dates between June 15 and August 1, 1911. In Series II the mean values are obtained from the measures of 16 plates at each latitude, observed between August 10 and September 11, 1911. In Series III at each of the three latitudes 0°, 30°, 60°, 24 plates were measured which were observed between October 3 and October 9, 1911. In Series I the 19 lines given in the preceding tables were measured on 14 of the plates. On the remaining 5 and on the 18 at 80° and 85° latitudes, 8 of the best defined lines only were measured. This number was reduced to diminish the labor of measurement, as the measures of the 14 plates were sufficient to show, as will be seen later, that any differences of rotational value for different elements were accidental in character. Furthermore, even with the reduced number of lines, the probable error of a plate as determined from the internal agreement among the lines was less than half the probable error obtained from the agreement between the plates. In Series II, however, owing to the higher probable error of measurement all the lines were measured

throughout and in Series III also on account of the systematic difference previously found for several of the lines by Adams.

In these summaries ϕ and V are the latitudes and reduced velocities at the points radially projected through the observed points to the limb (first correction method), and ϕ_1, ϕ_2, V_1, V_2 the latitudes and reduced velocities at the actually observed points (second correction method).

TABLE III

SUMMARY OF MEASURES

MEAN VALUES

Series I—λ 5600—Measured by Plaskett

Position Angle	Measured Velocity	ϕ	V	ϕ_1	V_1	ϕ_2	V_2	P. E. Plate	P. E. Mean
90° ...	1.824	0° 2′	2.017	1° 1′	2.018	0° 54′	2.018	±0.013	±0.003
75° ...	1.704	15 0	1.886	15 28	1.882	13 37	1.907	.027	.006
60° ...	1.493	29 58	1.652	30 5	1.652	27 56	1.698	.025	.006
45° ...	1.143	44 52	1.273	44 29	1.286	41 56	1.356	.042	.010
30° ...	0.723	59 46	0.809	58 40	0.842	55 16	0.935	.042	.010
15° ...	.363	74 28	.417	71 58	.487	66 44	.628	.026	.006
10° ...	.224	79 53	.247	75 13	.365	72 43	.426	.027	.006
5° ...	.113	84 47	.131	77 55	.310	74 58	.379	.020	.005
Means	....	....	....	....	....	....	....	±0.028	±0.006

Series II—λ 5600—Measured by DeLury

Position Angle	Measured Velocity	ϕ	V	ϕ_1	V_1	ϕ_2	V_2	P. E. Plate	P. E. Mean
90°....	1.659	0° 0′	1.950	2° 46′	1.949	2° 46′	1.949	±0.038	±0.009
74°8 ..	1.561	14 59	1.834	16 39	1.810	10 57	1.884	.058	.014
59°7 ..	1.416	29 53	1.654	30 30	1.650	24 16	1.775	.039	.010
44°5 ..	1.070	44 58	1.251	44 14	1.274	36 56	1.457	.052	.013
29°....	0.633	59 47	0.752	57 31	0.809	48 16	1.061	.039	.010
13°....	.301	74 31	.386	69 25	.525	57 6	0.859	.041	.010
Means	....	....	....	....	....	....	....	±0.044	±0.011

Series III—λ 4250—Measured by Plaskett

Position Angle	Measured Velocity	ϕ	V	ϕ_1	V_1	ϕ_2	V_2	P. E. Plate	P. E. Mean
90°....	1.754	0° 0′	2.012	2° 10′	2.013	2° 10′	2.013	±0.018	±0.004
59°8 ..	1.424	29 59	1.625	30 33	1.616	25 39	1.725	.020	.004
29°5 ..	0.688	59 53	0.788	58 25	0.834	50 55	1.037	.039	.008
Means	....	....	....	....	....	....	....	±0.026	±0.005

COMPARISON OF MEASURES

13. Before discussing the velocity and the law of its variation with the latitude it is desirable to attempt an explanation of the systematic difference between the values of the velocity obtained

by Plaskett and DeLury. Although the plates used are not the same, the difference persists when the same plates are measured by the two observers, as will be seen later. In the early measures of rotation plates the fields of the measuring microscopes were left unmasked, but later, as the quantity of light getting through was very fatiguing to the eye, diaphragms were arranged to cut out part of the illumination. This was effected in the case of the Repsold measuring machine used by Plaskett by placing a thin disk with three slots cut in it in the eyepiece of the micrometer just above the focal plane. By this means only the three strips of spectra were visible, the light coming through on the outside and between the strips being occulted by the disk. In the case of the Toepfer machine used by DeLury a single slit of the width of one of the spectral strips was cut in a brass plate which was held by a movable arm attached to the rigid microscope carriage close to the plate and which could be, by a convenient screw, readily moved transversely, so that, in measuring, only one of the three strips of spectrum could be seen at a time. This latter arrangement was devised by DeLury to keep the configuration of the spectrum lines the same for each measurement because he found that his measures were affected by the configuration of the lines in the three strips;[1] and further, to keep himself in ignorance of the magnitude and direction of the displacement so that his measures could in no way be affected by prejudice. For this latter reason also he postponed his computations of the velocities until all the measurements of his series were completed. On the other hand, as only part of the line in the strip being measured can be seen distinctly at one time, as the eye has to move up and down to make the best placing of the wire, it was felt by Plaskett that as the other strips could not be seen while the setting was being made, they could not influence it in any way. Consequently the simpler expedient of a fixed diaphragm occulting only the extraneous light was deemed sufficient by him. This is corroborated by the fact that the difference between Plaskett and DeLury is practically constant at all latitudes (except the pole), although the relative displacement of the lines in the spectra varies widely. On the other hand, the measures of

[1] *Jour. Roy. Astron. Soc. Can.*, **5**, 384–407.

15 equatorial spectra by DeLury both with and without the mask gave a systematically greater value for the former of 0.012 km on the average. It is an open question, since these measures were made at different epochs, whether this difference is to be ascribed to the use of the mask or to a change in the habit of measurement. The measures were made with great care by both observers and in precisely the same way: four settings on the line in the center strip, two on each of the outside strips with the screw moving alternately forward and back, and, after all the lines were measured, repeated with the plate reversed on the carriage. Moreover, as the measures are purely differential (the displacement of one absorption line with respect to another presumably similar absorption line), the presence of this comparatively large systematic difference between the two observers is not readily explainable. Different methods of measurement and various comparisons were made in an attempt to explain or overcome the difficulty, but the difference still persisted practically unchanged in magnitude and sign throughout.

14. It is proposed[1] by DeLury, in order to obtain absolute values of the displacement, which are uncertain under present circumstances, to impress upon the spectra, in addition to the rotation displacement, an arbitrary displacement of, say, the order of a millimeter in magnitude. This would be effected by using a double or broken slit, the central section (of the width of one of the spectral strips) being displaced laterally any desired distance with respect to the body of the slit. If a rotation spectrum be made through such a slit, the displacement will be $S+r$ where r is the rotation displacement and S the displacement due to the slit. If a spectrum of the limb at the pole where there is no rotational displacement be made through this slit the displacement will be S. The measured values of these displacements will be $S+r+e$ and $S+e$ where e is the error of measurement, varying with different observers, yet which should (for each observer) have the same value *in the mean* of a large series of measures, as the two displacements are relatively of nearly the same magnitude. The true value of the rotational displacement will then be

$$S+r+e-(S+e)=r$$

[1] *Ibid.*, **5**, 405.

and in this result personal habits of measurements should be eliminated. Besides the mechanical and observational difficulties in the way of this proposal, however, there is the further one that the accidental error of measurement would be increased and the amount of measuring required doubled. Furthermore, as these spectra could not be taken under identical conditions the possibility of instrumental errors affecting the results is rather a serious one. Even with rotation spectra made directly following one another, on the same plate, and under apparently identical conditions, such errors creep in, as for example in the equator plates of Series III. In plates 860, 865, 867, 869, the differences in the displacements of successive exposures are 0.066, 0.074, 0.051, 0.051 km per second, greater differences than the one in question. Consequently, although the method will be tried later, it was not deemed desirable to delay further the publication of the values obtained but to determine if possible the probable corrections to be applied to the velocities given above.

15. For this purpose all the equator spectra of Series I and 7 of Series III were measured by DeLury and all of Series II by Plaskett to determine systematic differences at the equator. In addition, to see how this difference varied with the latitude, 5 complete plates (7 latitudes on each) of Series I were measured by DeLury and 5 complete plates of Series II by Plaskett. Two representative plates of Series I, Nos. 813 and 820, were sent to Mt. Wilson and were kindly measured by Mr. Adams and Miss Lasby in order to compare Ottawa and Mt. Wilson measures. A tabulation of these comparisons, the measures of No. 820 being given in detail, not only serves to show the differences in velocity obtained by different measurers from the same plates, which appear to be generally systematic in character, but indicates also the accidental errors of measurement to be looked for. The measures of plates 813 and 820 show the great differences in accuracy of setting, for the probable error of setting on a single line varies on the average from 0.008 by Miss Lasby to 0.019 by Adams and Plaskett and to 0.052 km per second by DeLury, equivalent in linear values to 0.0004, 0.001, and 0.003 mm.

TABLE IV

COMPARISONS OF MEASURES. PLATES AT EQUATOR

SERIES I

Plate	Plaskett	DeLury (Mask)	Diff. P−D
772	1.812	1.770	0.042
777	1.840	1.814	.026
779	1.850	1.839	.011
782	1.854	1.832	.022
784	1.818	1.740	.078
787	1.848	1.786	.062
789	1.776	1.770	.006
796	1.841	1.805	.036
804	1.833	1.748	.085
813	1.858	1.845	.013
814	1.801	1.794	.007
817	1.839	1.823	.016
819	1.809	1.784	.025
820	1.792*	1.750*	.042
821	1.806	1.744	.062
822	1.800	1.734	.066
826	1.800	1.709	.091
827	1.815	1.713	.102
831	1.840	1.823	.017
Means	1.823	1.780	0.043

SERIES II

Plate	Plaskett	DeLury (No Mask)	DeLury (Mask)	Diff.	Diff. P−D (Mask)
833	1.770	.....	1.715	.....	0.055
834	1.807	1.765	1.771	6	.036
836	.....	1.549	1.565	16	.....
837	1.660	1.608	1.615	7	.045
838	1.694	1.651	1.664	7	.030
839	1.703	1.651	1.682	31	.021
842	1.731	1.644	1.651	7	.080
843	1.786	1.732	1.763	31	.023
844	1.709	1.690	1.671	−19	.038
845	1.695*	1.611*	1.620	9	.075
846	1.669	1.639	1.626	−13	.043
847	1.694	1.574	1.605	31	.089
848	1.705	1.632	1.643	11	.062
849	1.638	1.580	1.619	39	.019
851	1.772	1.691	1.705	14	.067
852	1.679	1.607	1.616	9	.063
Means	1.711	1.642	1.658	0.012	0.050

SERIES III

Plate	Plaskett	DeLury	Diff. P−D
859	1.764	1.791	−0.027
860	1.733	1.721	.012
861	1.780	1.776	.004
865	1.710	1.674	.036
866	1.774	1.768	.006
867	1.766	1.716	.050
869	1.719	1.752	−.033
Means	1.749	1.742	+0.007

* Mean values.

TABLE V

Comparisons of Measures. Plates with All Latitudes

Series I

Plate	Observer	0° Measures	0° Diff.	15° Measures	15° Diff.	30° Measures	30° Diff.	45° Measures	45° Diff.	60° Measures	60° Diff.	75° Measures	75° Diff.	90° Measures	90° Diff.
813	P	1.858		1.645		1.468		1.193		0.643		0.351		−0.001	
813	D	1.827	+31	1.627	+18	1.456	+ 12	1.170	+23	.635	+ 8	.331	+20	+ .034	−35
814	P	1.801		1.670		1.455		1.171		.692		.370		+ .029	
814	D	1.805	− 4	1.599	+71	1.402	+ 53	1.117	+54	.670	+22	.343	+27	+ .063	−34
817	P	1.839		1.651		1.520		1.195		.771		.373		− .006	
817	D	1.828	+11	1.578	+73	1.458	+ 62	1.138	+57	.743	+28	.362	+11	+ .017	−23
819	P	1.809		1.722		1.440		1.134		.689		.304		− .044	
819	D	1.787	+22	1.767	−45	1.452	− 12	1.118	+16	.678	+11	.318	−14	+ .081	+37
820	P	1.799		1.702		1.504		1.125		.652		.415		+ .001	
820	D	1.757	+42	1.675	+27	1.404	+100	1.055	+70	.676	−24	.345	+70	+ .006	− 7
Mean diffs. P–D			+20		+29		+ 43		+44		+ 9		+23		−12

Series II

Plate	Observer	0° Measures	0° Diff.	15° Measures	15° Diff.	30° Measures	30° Diff.	45° Measures	45° Diff.	60° Measures	60° Diff.	75° Measures	75° Diff.	90° Measures	90° Diff.
839	P	1.683		1.501		1.372		1.156		0.696		0.421		+0.056	
839	D	1.682	+ 1	1.529	−28	1.320	+ 52	1.126	+30	.681	+15	.419	+ 2	+ .015	+41
842	P	1.717		1.519		1.499		1.132		.723		.247		− .007	
842	D	1.651	+66	1.454	+65	1.482	+ 17	1.065	+67	.682	+41	.246	+ 1	− .028	+21
843	P	1.786		1.593		1.398		1.087		.624		.315		+ .025	
843	D	1.763	+23	1.580	+13	1.369	+ 29	1.050	+37	.595	+29	.270	+45	+ .043	−18
844	P	1.709		1.595		1.425		1.059		.630		.261		+ .001	
844	D	1.671	+38	1.568	+27	1.371	+ 54	1.064	− 5	.588	+42	.246	+15	− .020	+21
845	P	1.679		1.601		1.410		1.048		.619		.230		+ .023	
845	D	1.633	+46	1.574	+27	1.411	− 1	1.017	+31	.569	+50	.213	+17	+ .011	+12
Mean diffs. P–D			+35		+21		+ 30		+32		+35		+16		+15
Mean diffs. Series I and II			+27		+25		+ 36		+38		+22		+20		+ 1

TABLE VI

COMPARISON OF OTTAWA AND MT. WILSON MEASURES

Plate 813

Latitude	Mean Measured Velocities			Probable Error Single Line		
	Plaskett	DeLury	Lasby	Plaskett	DeLury	Lasby
0°	1.858	1.827	1.854	±0.010	±0.043	±0.007
15	1.645	1.627	1.710	.021	.068	.010
30	1.468	1.456	1.479	.015	.046	.012
45	1.193	1.176	1.130	.019	.060	.009
60	0.643	0.635	0.644	.023	.059	.005
75	.351	.331	.334	.025	.050	.006
Means	....	....	....	±0.019	0.054	0.008

Plate 820

No. of Line	0°						15°		
	Plaskett 1	Plaskett 2	DeLury 1	DeLury 2	Lasby	Adams	Plaskett	DeLury	Lasby
1	1.852	1.823	1.818	1.869	1.850	1.797	1.725	1.784	1.758
2	1.849	1.814	1.721	1.622	1.847	1.821	1.729	1.635	1.746
3	1.790	1.854	1.711	1.836	1.851	1.876	1.739	1.790	1.778
4	1.753	1.783	1.823	1.823	1.854	1.817	1.687	1.508	1.761
5	1.824	1.847	1.812	1.699	1.841	1.785	1.746	1.719	1.790
6	1.826	1.776	1.729	1.725	1.868	1.779	1.688	1.655	1.751
7	1.783	1.744	1.808	1.762	1.861	1.806	1.696	1.692	1.759
8	1.810	1.765	1.733	1.757	1.861	1.774	1.696	1.697	1.767
9	1.795	1.782	1.671	1.682	1.855	1.800	1.696	1.695	1.742
10	1.780	1.770	1.727	1.854	1.854	1.770	1.696	1.642	1.758
11	1.764	1.754	1.628	1.693	1.855	1.776	1.713	1.676	1.765
12	1.830	1.780	1.707	1.637	1.871	1.793	1.712	1.621	1.762
13	1.787	1.749	1.746	1.805	1.839	1.786	1.721	1.635	1.755
14	1.823	1.805	1.804	1.779	1.851	1.808	1.699	1.745	1.765
15	1.806	1.824	1.787	1.818	1.848	1.855	1.705	1.759	1.749
16	1.756	1.746	1.663	1.709	1.841	1.767	1.650	1.606	1.763
17	1.778	1.781	1.677	1.788	1.835	1.767	1.653	1.613	1.762
18	1.760	1.751	1.735	1.745	1.856	1.785	1.706	1.769	1.743
19	1.808	1.749	1.827	1.721	1.829	1.802	1.685	1.576	1.746
Means	1.799	1.784	1.744	1.757	1.851	1.798	1.702	1.675	1.758
P.E. line	±0.020	0.023	0.043	0.047	0.007	0.019	0.017	0.052	0.008

16. The comparison of plates at the equator shows a systematic difference for measures of the same plates of 0.046 km per second. When the five complete plates of Series I and II are compared it is found that in these plates the average difference at the equator

is smaller about 0.027 and that this remains unchanged practically for all latitudes except the pole. This shows that the difference is evidently not due to any effect of the magnitude of the displacement of the lines of one strip with respect to the other, else it should vary with the latter, which changes from 0.1 mm at equator to about 0.017 mm at 75°. It may be said therefore that Plaskett measures the displacements from 0.03 to 0.05 km per second higher than DeLury in the region at λ 5600. The peculiar nature of the difference P—D at the pole should not pass without comment. The mean value of this difference is —0.001. Although it is of the same sign as the other differences in the Series II plates, it is of the opposite sign in Series I and is hence not systematic as at the other latitudes, and it might therefore be regarded as evidence that the magnitude or sense of the displacement influences the measures of one or both of the observers. Owing to the method of measurement used by DeLury, he would seem to be less likely to be influenced in this way. When we compare the measures in the λ 4250 region we find that the difference found in the λ 5600 region nearly vanishes, being only 0.007 km, scarcely large enough, considering the few plates measured by DeLury, to be deemed systematic. The spectra in the λ 4250 region are much more easily measurable than at λ 5600. Not only is the grain of the plate finer but the lines themselves are much more uniform in character and better defined. Consequently it seems likely that the large difference between the two measures in the λ 5600 region depends in some way upon the character of the lines for measurement. Although the probable error of measurement of a single line, given for plates 813 and 820 above, for Plaskett is only about a third of that for DeLury, ±0.019 and 0.054 km per second, and hence the former's measures should be considered of greater weight, yet that does not settle the question of the correct value of the velocity. Possibly some information may be obtained from the Mt. Wilson measures.

17. Mr. Adams and Miss Lasby have had greater experience than anyone else in the measurement of photographic rotation spectra and their measurements should be given great weight. Yet

when we come to make comparisons, Table VI, plates 813 and 820, we find practically the same difficulties and the same differences as between the writers. For example, in plate 820 at the equator we have Miss Lasby's value 1.851, Mr. Adams' 1.798, Plaskett's 1.799 and 1.784, DeLury's 1.757 and 1.744. Indeed, in several cases Miss Lasby's value is as much higher than Plaskett's as his is than DeLury's. On the other hand, in plate 813, 45°, it is lower than both and in plate 813, 60° and 75°, all three are practically the same. When we compare these differences with the probable error of measurement of the plates, less than one-quarter of the probable errors of single lines, varying from 0.002 to 0.015, we are forced to the conclusion that they are systematic and personal in nature, but are at a loss to account for their cause.

It is unfortunate that Mr. Adams was unable to measure more than the one spectrum but the close agreement of his result with Plaskett's and the generally higher values of Miss Lasby and lower of DeLury would naturally, from the law of averages, lead to the acceptance of Adams' and Plaskett's measures as probably being nearest to the true values. If such a conclusion be accepted, then it would be necessary to apply a positive correction to DeLury's measures in the λ 5600 region, which, when all the comparisons are taken into account, should be about 0.040 km at the equator and possibly slightly less at the higher latitudes. A further evidence that this is probably the proper course is given by the practical agreement of Plaskett's and DeLury's measures in Series III at λ 4250. As the velocities of rotation obtained by Plaskett from the measures of Series I, II, and III are all practically the same, while those obtained by DeLury are about 3 per cent lower for Series I and II but the same for Series III, the inference is that, in the poorer quality lines in the yellow green, some personal effect causes the difference and that this disappears when the lines become better defined as is the case in the violet. On the other hand, if there be no systematic differences in the measuring of the line displacements by DeLury at the two regions λ 4250 and λ 5600 this would imply a difference in the rates of rotation as determined from lines of different wave-length, a thing which, though in itself not impossible, is perhaps not very probable.

ABSOLUTE VALUE OF VELOCITY. VARIATION OF VELOCITY WITH LATITUDE

18. The above discussion and comparison of measures have shown that it is hardly possible to state exactly the absolute velocity of the rotation of the sun and, furthermore, if, as seems likely, earlier determinations were affected in the same way, they are also uncertain to the same extent, that of the "personal equation" of measurement.

In order to place the preceding summaries of measures in a more convenient form for discussion and comparison, the following tables containing the observed mean linear velocities at the mean latitudes have been compiled. From these linear velocities the observed angular velocities have been directly computed, while the other columns will be explained below.

TABLE VII

SUMMARY

Series I

Latitude	Linear Velocities			Angular Velocities		
	Observed	Computed	Residual (O.−C.)	Observed	Computed	Residual (O.−C.)
0° 2'.....	2.017	2.014	+0.003	14°.32	14°.40	−0°.08
0 54......	2.018	2.014	+ .004	14.33	14.40	− .07
1 1......	2.018	2.014	+ .004	14.33	14.40	− .07
13 37......	1.907	1.928	− .021	13.93	14.17	+ .24
15 0......	1.886	1.910	− .024	13.86	14.12	+ .26
15 28......	1.882	1.905	− .023	13.86	14.10	+ .24
27 56......	1.698	1.679	+ .019	13.64	13.50	+ .14
29 58......	1.652	1.632	+ .020	13.54	13.38	+ .16
30 5......	1.652	1.630	+ .022	13.55	13.37	+ .18
41 56......	1.356	1.328	+ .028	12.94	12.58	+ .36
44 29......	1.286	1.258	+ .028	12.80	12.40	+ .40
44 52......	1.273	1.246	+ .027	12.75	12.38	+ .37
55 16......	0.935	0.951	− .016	11.65	11.66	− .01
58 40......	.842	.854	− .012	11.50	11.44	+ .06
59 46......	.809	.823	− .014	11.41	11.37	+ .04
66 44......	.628	.625	+ .003	11.29	10.97	+ .32
71 58......	.487	.481	+ .006	11.17	10.73	+ .44
72 43......	.426	.460	− .034	10.18	10.70	− .52
74 28......	.417	.413	+ .004	11.06	10.66	+ .40
74 58......	.379	.399	− .020	10.37	10.62	− .25
75 13......	.365	.392	− .027	10.16	10.61	− .45
77 55......	.310	.320	− .010	10.51	10.53	− .02
79 53......	.247	.267	− .020	9.98	10.46	− .48
84 47......	.131	.137	− .006	10.23	10.37	− .14

TABLE VII—*Continued*

Series II

Latitude	Linear Velocities Observed	Computed	Residual (O.−C.)	Angular Velocities Observed	Computed	Residual (O.−C.)
0° 0′	1.950	1.971	−0.021	13°84	14°04	−0°20
2 45	1.949	1.969	− .020	13.85	14.03	− .18
10 56	1.884	1.917	− .033	13.62	13.89	− .27
14 59	1.834	1.870	− .036	13.48	13.78	− .30
16 40	1.810	1.847	− .037	13.41	13.72	− .31
24 16	1.775	1.716	+ .059	13.82	13.37	+ .45
29 53	1.654	1.596	+ .058	13.54	13.04	+ .50
30 30	1.650	1.583	+ .067	13.60	13.01	+ .59
36 56	1.457	1.424	+ .033	12.94	12.60	+ .34
44 14	1.274	1.230	+ .044	12.62	12.10	+ .52
44 48	1.251	1.215	+ .036	12.52	12.06	+ .46
48 23	1.061	1.115	− .054	11.08	11.81	− .73
57 6	0.859	0.871	− .012	11.23	11.23	0
57 32	.809	.859	− .050	10.70	11.20	− .50
59 47	.752	.796	− .044	10.61	11.06	− .55
69 26	.525	.532	− .007	10.61	10.54	+ .07
74 31	.386	.397	− .011	10.37	10.33	+ .04

Series III

Latitude	Linear Velocities Observed	Computed	Residual (O.−C.)	Angular Velocities Observed	Computed	Residual (O.−C.)
0° 0′	2.012	2.020	−0.008	14°28	14°33	−0°05
2 10	2.013	2.018	− .005	14.30	14.33	− .03
25 39	1.725	1.720	+ .005	13.59	13.54	+ .05
29 59	1.625	1.619	+ .006	13.32	13.27	+ .05
30 33	1.616	1.605	+ .011	13.32	13.23	+ .09
50 55	1.037	1.046	− .009	11.68	11.78	− .10
58 25	0.834	0.830	+ .004	11.30	11.26	+ .04
59 53	.788	.789	− .001	11.15	11.16	− .01

19. From these mean values, about one-third of which are due to Method I of reduction and two-thirds to Method II, the law of variation of latitude has to be obtained. Many different forms containing both sine and cosine terms of the latitude in different powers were tried and, although some gave close agreement, none on the whole were as good as the simple Faye formulae

$$V=(a+b\cos^2\phi)\cos\phi$$
$$\xi=a'+b'\cos^2\phi.$$

Using the method of least squares to determine the constants, the following formulae were obtained:

$$\text{Series I} \begin{cases} V=(1.504+0.509\cos^2\phi)\cos\phi \\ \xi=10^\circ\!.34+4^\circ\!.06\cos^2\phi \end{cases}$$

$$\text{Series II} \begin{cases} V=(1.448+0.523\cos^2\phi)\cos\phi \\ \xi=10^\circ\!.04+4^\circ\!.00\cos^2\phi \end{cases}$$

$$\text{Series III} \begin{cases} V=(1.421+0.599\cos^2\phi)\cos\phi \\ \xi=10^\circ\!.10+4^\circ\!.23\cos^2\phi \end{cases}$$

From these formulae the values in columns headed "Computed" and "Residual" in the preceding tables, Table VII, were obtained. The residuals in Series I and III are satisfactorily small and show no tendency to systematic arrangement of sign. In Series II, however, they are considerably larger and systematically grouped as to sign, indicating the necessity of an additional term in the Faye formula.

If the observations of Series I and III are grouped together we get formulae which represent the observations in both series nearly as well as the separate formulae. The difference between the formulae for Series I and III above is probably due to the small number of latitudes observed (only three) in Series III, in which case a small deviation of one of the values would make a large change in the coefficients. The formulae from both series

$$\text{Series I and III (combined)} \begin{cases} V=(1.483+0.532\cos^2\phi)\cos\phi \\ \xi=10^\circ\!.32+4^\circ\!.05\cos^2\phi \end{cases}$$

may therefore be considered as the formulae obtained from Plaskett's measurements. Series II is not included in this on account of the systematic difference and because another term would be necessary to obtain reasonable agreement between the observed and computed values. However, if we compare the coefficients from Series II with those from Series I and III combined, we find them practically the same except for the difference in the first terms, which is in line with what has been found by comparison of the measures. Moreover this difference, when the necessary allowance is made for the difference of the coefficients of the second terms, is 0.044 km or $0^\circ\!.33$, which is not far from the assumed 0.040 km.

20. For convenience of comparison the previously obtained formulae are tabulated beside those just given and we at once notice a remarkable similarity between the Ottawa and Mt. Wilson coefficients.

TABLE VIII

FORMULAE FOR SOLAR ROTATION

Observer	Linear Velocities	Angular Velocities
Dunér..................	....	$10^\circ.60+4^\circ.21 \cos^2\phi$
Halm..................	....	$12.03+2.50 \cos^2\phi$
Adams (1906–1907)......	$(1.575+0.480 \cos^2\phi) \cos\phi$	
Adams (1908)..........	$(1.507+0.546 \cos^2\phi) \cos\phi$	$10.57+4.04 \cos^2\phi$
Adams (mean)..........	$(1.550+0.501 \cos^2\phi) \cos\phi$	$11.04+3.50 \cos^2\phi$
Plaskett (1911).........	$(1.483+0.532 \cos^2\phi) \cos\phi$	$10.32+4.05 \cos^2\phi$
DeLury (1911)	$(1.448+0.523 \cos^2\phi) \cos\phi$	$10.04+4.00 \cos^2\phi$

This is especially the case with the 1908 Mt. Wilson determination and the mean formulae from Series I and III, where, in the angular form, the difference is only in the constant term. In the linear form also they are quite similar and their agreement in both forms is so marked as compared with the widely different coefficients obtained from the 1906–1907 Mt. Wilson observations as to confirm the presence of some systematic error in the latter, suspected by Adams, and to indicate the substantial accuracy of the law of variations obtained.

The daily angular value of the rotational velocity has been computed from the empirical formulae given in the preceding table for the latitudes from the equator to the pole by intervals of 5°. A column containing the results of Storey and Wilson[1] at Edinburgh is added and a column for the velocities of sun-spots, the means from three formulae given in Adams'[2] work. Further, the linear velocities from Adams' 1908 and Plaskett's formulae have been computed and are given in the last two columns (Table IX).

The agreement of Dunér's, Adams' 1908, and the Ottawa values except for small and nearly constant angular differences is quite striking and gives good grounds for the belief that the law of variation with latitude is represented to a high degree of accuracy by a Faye formula with coefficients approximately the same as those given in these three formulae.

21. In regard to the absolute value of the rotational velocity the question cannot be regarded as by any means settled. Considering the velocity values at the higher latitudes, we find that

[1] *M.N.*, **62**, p. 674.

[2] Adams and Lasby, p. 118.

TABLE IX

VELOCITIES OF ROTATION

LATITUDE	DAILY ANGULAR VELOCITIES								LINEAR VELOCITIES	
	Sun-Spots	Dunér	Halm	1906-1907 Adams	1908 Adams	1908-1910 Storey and Wilson	1911 Plaskett	1911 DeLury	1908 Adams	1911 Plaskett
0°	14°40	14°81	14°53	14°63	14°61	14°81	14°37	14°05	2.053	2.015
5	14.38	14.78	14.50	14.59	14.58	14.72	14.34	14.02	2.041	2.003
10	14.31	14.68	14.46	14.50	14.49	14.59	14.25	13.93	2.007	1.968
15	14.20	14.53	14.37	14.37	14.34	14.46	14.10	13.78	1.948	1.912
20	14.06	14.32	14.24	14.17	14.13	14.32	13.89	13.58	1.869	1.835
25	13.89	14.06	14.09	13.94	13.89	14.15	13.65	13.34	1.772	1.740
30	13.69	13.76	13.90	13.67	13.60	13.97	13.36	13.05	1.659	1.630
35	13.47	13.42	13.70	13.39	13.28	13.74	13.04	12.73	1.535	1.508
40	.....	13.07	13.50	13.09	12.94	13.52	12.70	12.40	1.400	1.375
45	.....	12.70	13.28	12.81	12.58	13.26	12.34	12.05	1.259	1.237
50	.....	12.34	13.07	12.54	12.24	13.01	11.99	11.70	1.113	1.093
55	.....	11.99	12.86	12.30	11.91	12.71	11.65	11.37	0.967	0.950
60	.....	11.65	12.66	12.11	11.58	12.43	11.33	11.05	.821	.807
65	.....	11.35	12.48	11.97	11.29	12.04	11.04	10.76	.677	.666
70	.....	11.09	12.32	11.91	11.05	11.64	10.80	10.52	.538	.528
75	.....	10.88	12.20	11.91	10.84	11.24	10.59	10.32	.399	.392
80	.....	10.74	12.11	12.00	10.69	.....	10.44	10.17	.265	.260
85	.....	10.63	12.05	12.17	10.60	.....	10.35	10.08	.130	.128
90	.....	10.60	12.03	12.43	10.57	.....	10.32	10.05	0	0

Halm and Adams get nearly the same values, Dunér and Storey and Wilson are about 1 per cent higher, Plaskett about 2 per cent lower, and DeLury about 4 per cent lower. But at the higher latitudes Dunér and Adams (1908) agree, Plaskett is 2 per cent lower as before, DeLury about 5 per cent lower, Storey and Wilson are 5 per cent higher, while Halm and Adams (1906–1907) are some 15 or 20 per cent higher. At the equator Plaskett's values are in practical agreement with the motion of sun-spots. As it is generally considered that the reversing layer and sun-spots are at the same level from the practical identity of their spectra, this, so far as it goes, gives weight to the lower value of 14°4 at the equator. On the other hand, as the latitude increases the sun-spot velocities agree better with the higher values of the reversing layer such as those of Halm and of Adams' 1906–1907 observations.

22. These differences in values may be due to one or more of three causes: (*a*) a variation in the rate of rotation of the sun; (*b*) instrumental errors; (*c*) personal errors of measurement.

a) *Variation in rate of rotation of sun.*—The question of a change in the rotational velocity of the sun, which was raised by Halm,[1] was quite fully discussed by Adams,[2] who reached the conclusion that the evidence to date was against variation. The later values by Storey and Wilson and those obtained here of which the former is higher and the latter lower than Adams' results, would indicate a variation in the rate of rotation were it not for the possibility of small instrumental and the probability of personal measurement errors (Sections 15–17). As it is, until the latter are eliminated, it will be impossible to make any definite statement in regard to either the variation or constancy of the rate. Certainly the possibility of a variation must, until further evidence is available, be taken into account in considering the differences obtained.

b) *Instrumental errors.*—So far as instrumental errors are concerned, although every known precaution was taken to avoid them, it is possible that some small systematic effects may be present in these results. The only means of detecting such an error would be by the comparison of spectra made at the same epoch by different instruments and methods and measured by the same observer but such is not easy to arrange. The differences in value for successive plates taken under, so far as known, identical conditions (previously referred to in Section 16) is most likely due to some sort of instrumental error unless rapid changes in local motions in the reversing layer are responsible. Although these differences are apparently quite accidental, they may nevertheless contain a small systematic deviation.

c) *Personal errors of measurement.*—It has been shown (Sections 15–17) that it is possible, even probable, for such differences as those in question to be obtained on measurement of the same plate by different observers and it seems useless to consider other sources of error until it is possible to eliminate this. Although the difference between Plaskett and DeLury is fairly well determined at λ 5600 as, at present, about 0.04 km per second, sufficient plates in common have not yet been measured to determine the difference between Miss Lasby, by whom most of the Mt. Wilson plates were measured, and the writers. Her measures appear to be somewhat higher on the whole (Section 17) than Plaskett's and the same

[1] *A.N.*, **173**, p. 294.

[2] Adams and Lasby, p. 115.

tendency was shown even more markedly during a visit of the latter to Mt. Wilson in 1910, where comparisons of the measured displacements of several lines on rotation plates at the equator showed that Miss Lasby's measures were always 2 or 3 per cent higher than Plaskett's. If there is this difference, then the actual velocity displacements on the Mt. Wilson and Ottawa plates are approximately the same and it only remains to determine whose measurement is the most nearly correct. At present, however, we shall have to be satisfied with recognizing the presence of personal differences of measurement, as accounting for part at any rate of the differences in velocity obtained.

23. In view of these actual differences of velocity obtained by the different observers and after the discussion of the probable causes of these differences, we can only state that the velocity of the solar rotation as determined from Plaskett's measurements is represented by the formulae

$$V=(1.483+0.532\cos^2\phi)\cos\phi$$
$$\xi=10^\circ\!.32+4^\circ\!.05\cos^2\phi$$

and that the angular form differs from Adams' 1908 formula practically only in the constant term and is also in good agreement with Dunér's, and that hence it probably represents very closely the relative velocities at the different latitudes, although the absolute values may be uncertain by, say, 2 per cent.

PROBABLE ERRORS

24. As Adams[1] has already compared his errors of measurement with those of Dunér and Halm, showing the marked advantage of the photographic method, it will suffice here to give the Ottawa values and compare them with Adams'.

The mean probable error of measurement of the velocity from a single line determined by the use of *all* the lines on *all* the plates is

Series I = ±0.024 km per sec.
Series II = ±0.056 km per sec.
Series III = ±0.015 km per sec.

The probable errors in Series I vary for the different plates from 0.010 to 0.040 and in Series III from 0.006 to 0.023. As the

[1] Adams and Lasby, p. 117.

number of lines measured on each plate in the two series has been 19 and 15, respectively, the probable error of an average plate as determined from the internal agreement of the measures is

Series I = ±0.0055 km per sec.
Series II = ±0.012 km per sec.
Series III = ±0.0038 km per sec.

The average probable error of a plate determined from comparisons of the velocities of all plates at the same latitudes and for all the latitudes is

Series I = ±0.028 km per sec.
Series II = ±0.044 km per sec.
Series III = ±0.026 km per sec.

or 5, 4, and 7 times the probable error as determined from the internal agreement of the lines.

These somewhat anomalous results are not unusual, as about the same ratio of probable errors is obtained in stellar radial velocity work and in many other astrophotographic methods, but the cause of this comparatively high ratio cannot be satisfactorily explained. One can imagine that changing instrumental conditions might cause differences in displacement in plates taken on different dates but where, as in the example previously cited, differences of from 0.05 to 0.07 km were found on exposures taken one immediately after the other on the same plate on the same region of the sun and under, so far as known, identical conditions, no explanation, except the not very likely one of rapidly changing proper motions on the sun, can be assigned.

25. In comparing these probable errors with those of Mt. Wilson, only Series III, which is in the same region, λ 4250, as the Mt. Wilson plates, must be considered, for, as the relative probable errors indicate, the lines are of much better quality for measurement than at λ 5600. When the probable errors (in kilometers) are reduced to linear measure they become more than twice as great at λ 5600 as at λ 4250. The probable errors for a single line obtained at Mt. Wilson are

P.E. = ±0.015 km per sec. (1906–1907)
P.E. = ±0.009 km per sec. (1908).

The Ottawa value, as above stated, is ± 0.015. It must not be forgotten, however, that the Mt. Wilson values are from one or two plates, the Ottawa from the mean of all the plates; that on the Mt. Wilson plates the lines giving, systematically, velocities differing from the mean were excluded, on the Ottawa plates these and all lines were included; and lastly, that the Mt. Wilson linear dispersion was in 1906–1907, 10 per cent and in 1908, 30 per cent greater than that at Ottawa. Hence it is evident that the probable error of measurement is about the same at the two places. Although the probable error of a plate determined from the agreement among the plates is not given, it is readily computed and for the equator (1908) is ± 0.011 km per second as compared with ± 0.018 here. This is considerably smaller but yet about five times that obtained from agreement among the lines.

26. It is evident from the ratios of the probable errors that a great many more lines than necessary for the actual determination of the rotation have been measured and that it would be preferable to measure four or five times as many plates with only one-fourth or one-fifth the number of lines, and that even then the probable error obtained from comparison of the plates would be twice that deduced from the internal agreement of the lines. However, in this investigation a larger number of lines was measured for the purpose of determining whether different elements and different lines of the same element give different velocities of rotation.

SYSTEMATIC DIFFERENCES OF VELOCITY FOR DIFFERENT ELEMENTS

27. Considerable attention has been devoted to this phase of the investigation, which is of importance not only because of its interest in the theory of the sun but also because it was one of the questions proposed by the Rotation Committee and because Adams has found some small systematic differences for different elements and his results should be confirmed.

As previously mentioned, in the λ 5600 region the lines were chosen particularly with this point in view and include as large a number of elements as is possible among the limited number available for measurement. Similarly in the λ 4250 region, besides the 10 lines selected for measurement by the committee, 5 other

lines, embracing those found by Adams to give systematic deviations, were included.

28. The following table contains the mean residuals in meters per second obtained from Plaskett's measures of about 14 plates and DeLury's measures of 16 plates at λ 5600. The first three columns contain the wave-length, source, and intensity of the lines measured. The first column under each observer contains the number of measures on which the residuals in the next two columns depend; the second columns the average residual without regard to sign, and the last columns the mean residual when the sign is taken into account, or the algebraic residual.

TABLE X

MEAN RESIDUALS

λ 5600—Series I and II

Wave-Length	Element	Intensity	Plaskett, Series I			DeLury, Series II		
			Number of Measures	Mean (Numerical)	Mean (Algebraic)	Number of Measures	Mean (Numerical	Mean (Algebraic)
5506.095...	*Mn*	1	97	32	+ 3	112	85	+20
5514.563...	*Ti*	2	81	36	+ 1	112	82	+ 3
5514.753...	*Ti*	2	97	41	+ 9	112	71	− 3
5528.641...	*Mg*	8	97	27	− 9	112	83	− 6
5544.157...	*Fe*	2	97	28	− 5	112	66	− 6
5560.434...	*Fe*	2	97	32	+ 6	112	71	− 2
5562.933...	*Fe*	2	97	25	− 1	112	59	+11
5578.946...	*Ni*	1	97	31	+ 3	112	66	+18
5582.198...	*Ca*	4	97	25	− 3	112	48	+ 6
5590.343...	*Ca*	3	97	24	− 1	112	65	+ 6
5598.524...	*Fe*	1	81	26	− 1	112	65	− 2
5601.505...	*Ca*	3	96	22	+ 1	112	57	− 2
5624.769...	*Fe, V*	4	97	28	+ 9	112	59	−10
5638.488...	*Fe*	3	97	28	+ 5	112	59	+ 1
5658.097...	*V*	2	97	25	− 1	112	69	+10
5682.869...	*Na*	5	97	28	−14	112	65	−18
5684.710...	*Si*	3	97	27	+ 1	112	69	−21
5686.757...	*Fe*	3	81	28	0	112	63	+ 5
5688.436...	*Na*	6	97	25	− 1	112	62	− 2

The trend and magnitude of the mean residuals in Plaskett's measures for the different latitudes, which are, for lack of space, not given here, and the ratio of the mean algebraic to the mean numerical residual, which is except in one case less than one-third, do not indicate any systematic differences for the different lines.

If any lines or elements gave a different velocity than the mean reversing layer, then the mean residuals for the different latitudes for these lines should be of the same sign, should diminish as the latitude increased, and should vanish at the pole. We find on the contrary that none of the lines fulfil this condition but that the residuals bear the appearance of being quite accidental in character. Even in the case of the *Na* line 5682.869, which gives a strong negative residual, we find no decrease with higher latitudes and the mean residual for the pole is much higher than the average, showing that the difference is probably due to something in the line. Again if this sodium line did give a lower value of the velocity, the other sodium line, the last on the list, should also give a negative residual, whereas we see its residuals are entirely accidental. The same condition of affairs is shown by the tabulated residuals from DeLury's measures of Series II in which the mean algebraic is always less than one-fourth the mean numerical residual, although the numbers are higher owing to his higher probable error of measurement.

These considerations form sufficient grounds for the statement that in the region around λ 5600 none of these lines or elements give velocities differing from that of the general reversing layer to a greater extent than can readily be accounted for by accidental errors of measurement.

29. The same thing appears to be the case in the λ 4250 region. The following table contains the mean residuals in meters per second from the 15 lines measured on 24 plates at the equator, at 30° and at 60° latitude; and the final mean numerical and algebraic residuals for the whole 72 measures of each line (Table XI).

Again it will be noticed that in the final values no mean algebraic is one-third as large as the mean numerical residual, even though in three cases the algebraic mean for one of the latitudes is nearly as large as the corresponding numerical mean. The mean residuals obtained from Adams' 1908 values are given in the last column and those lines which Adams claimed gave lower or higher values than the general reversing layer are indicated by the letters L and H in the preceding column. It will at once be seen that the results obtained from the 72 plates measured by Plaskett do not

agree with those of Adams, but on the contrary are generally of the opposite sign. It seems to us therefore that the only safe conclusion to be drawn from the evidence at hand is that any differences found in both Adams' and Plaskett's values are not real differences of velocity but are, if not wholly accidental, rather some personal

TABLE XI

MEAN RESIDUALS—λ 4250

Wave-Length	Element	Intensity	Mean (Numerical)				Mean (Algebraic)					
			0°	30°	60°	All	0°	30°	60°	All		Adams
4196.699...	*La*	2	20	21	26	22	+9	+3	+8	+7	L	−14
4197.257...	*C*	2	17	22	25	21	+2	−2	+8	+3	L	−11
4216.136...	*C*	2	15	26	22	21	+3	+4	0	+2	L	−11
4220.509...	*Fe*	3	15	16	17	16	+6	−8	−8	−3	..	+4
4225.619...	*Fe*	3	17	24	21	27	−2	+18	0	+5	..	....
4232.887...	*Fe*	2	13	15	15	14	−4	−2	+1	−2	..	+2
4241.285...	*Fe, Zr*	2	19	17	14	17	+7	0	−6	0	..	....
4246.996...	*Sc*	5	15	19	21	18	−8	+6	+9	+2	..	....
4257.815...	*Mn*	2	15	19	15	16	+1	0	0	0	H	+9
4258.477...	*Fe*	2	17	17	20	18	−16	−2	−3	−7	..	+5
4266.081...	*Mn*	2	19	19	11	16	+2	−2	−6	−2	..	+7
4268.915...	*Fe*	2	16	15	20	17	−4	−7	−5	−5	..	+7
4276.836...	*Zr*	2	12	20	17	16	0	−14	0	−5	..	+2
4290.377...	*Ti*	2	14	16	17	16	−1	−1	−2	−1	L	−4
4291.630...	*Fe*	2	12	22	18	17	+2	−3	+3	+1	..	+6

effect in the measurement due possibly to the character of the line. The measures of arbitrary displacements[1] also support the conclusion that such differences as occur are due to errors of measurement. It is unfortunate that no plates containing H_α and *Ca* λ 4227 were obtained here in order to compare the rotational values obtained from these lines with the general reversing layer as was done by Adams, but it seems likely that personal differences at least as high as those occurring in the general reversing layer would be present in the measures of these broad and difficult lines.

SUMMARY

30. The principal conclusions reached from this investigation may be briefly summarized as follows:

[1] *Trans. Roy. Soc. Can.*, 1911, Sec. III, p. 116.

a) The Ottawa values of the solar rotation may be represented by the formulae

$$\left.\begin{aligned} V&=(1.483+0.532\cos^2\phi)\cos\phi \\ \xi&=10^\circ\!.32+4^\circ\!.05\cos^2\phi \end{aligned}\right\}\text{Plaskett}$$

$$\left.\begin{aligned} V&=(1.448+0.523\cos^2\phi)\cos\phi \\ \xi&=10^\circ\!.04+4^\circ\!.00\cos^2\phi \end{aligned}\right\}\text{DeLury}$$

which are in remarkably good agreement with Dunér's and Adams' 1908 results except for a small and nearly constant angular difference and which probably represent very closely the law of variation with latitude.

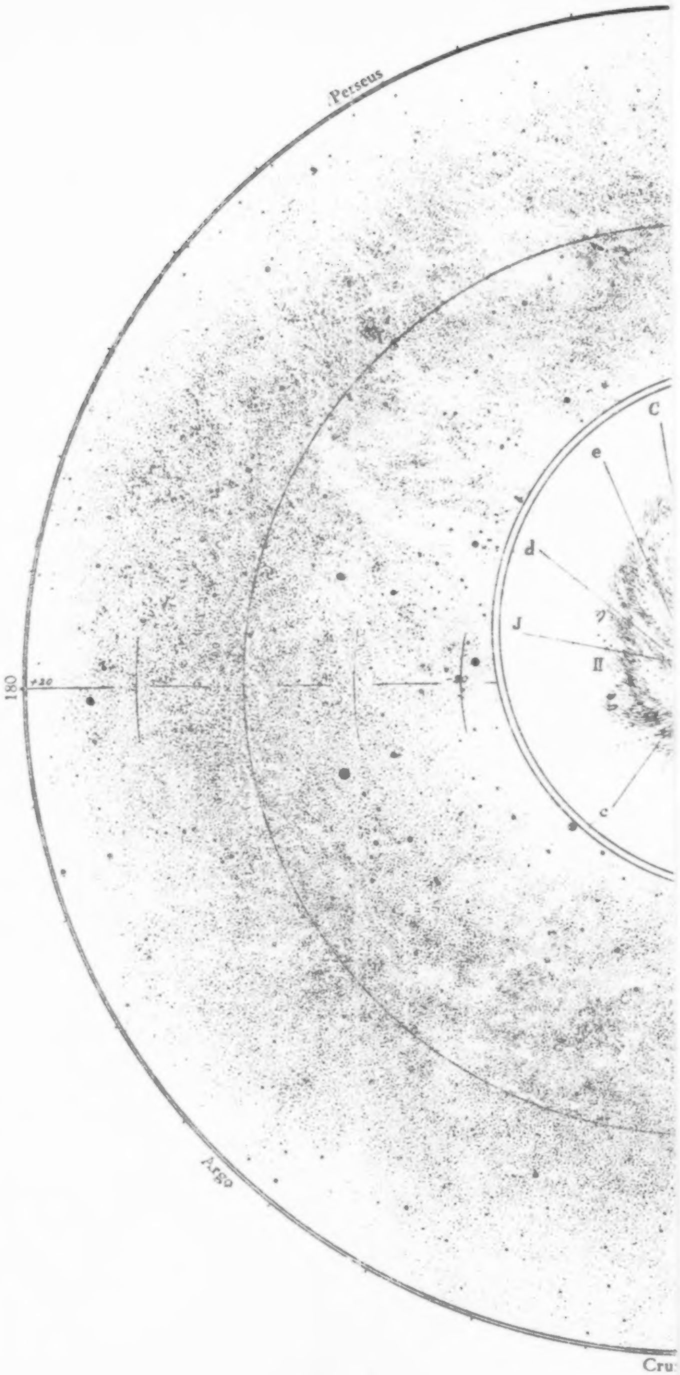
b) The absolute velocity of the solar rotation seems to be uncertain by the small difference above referred to, of the order of 2 or 3 per cent, which is apparently due to personal differences in the habit of measurement of the rotational displacements on the plates.

c) The tabulation and discussion of about 3,000 residuals from different lines and elements in the regions measured show that no systematic difference of velocity for different elements is present in the Ottawa plates. The frequently opposite signs of the mean residuals at Ottawa and Mt. Wilson from the same lines (those found at the latter place to give systematically higher or lower velocities) would point to the conclusion that the deviations previously found might have been either accidental or, more probably, personal and due to the character of the lines.

It gives us much pleasure to record here our appreciation of the interest the director, Dr. W. F. King, has taken in this work, of the help he has afforded, and of his willingness to meet the many needs in the matter of apparatus arising in the course of the work.

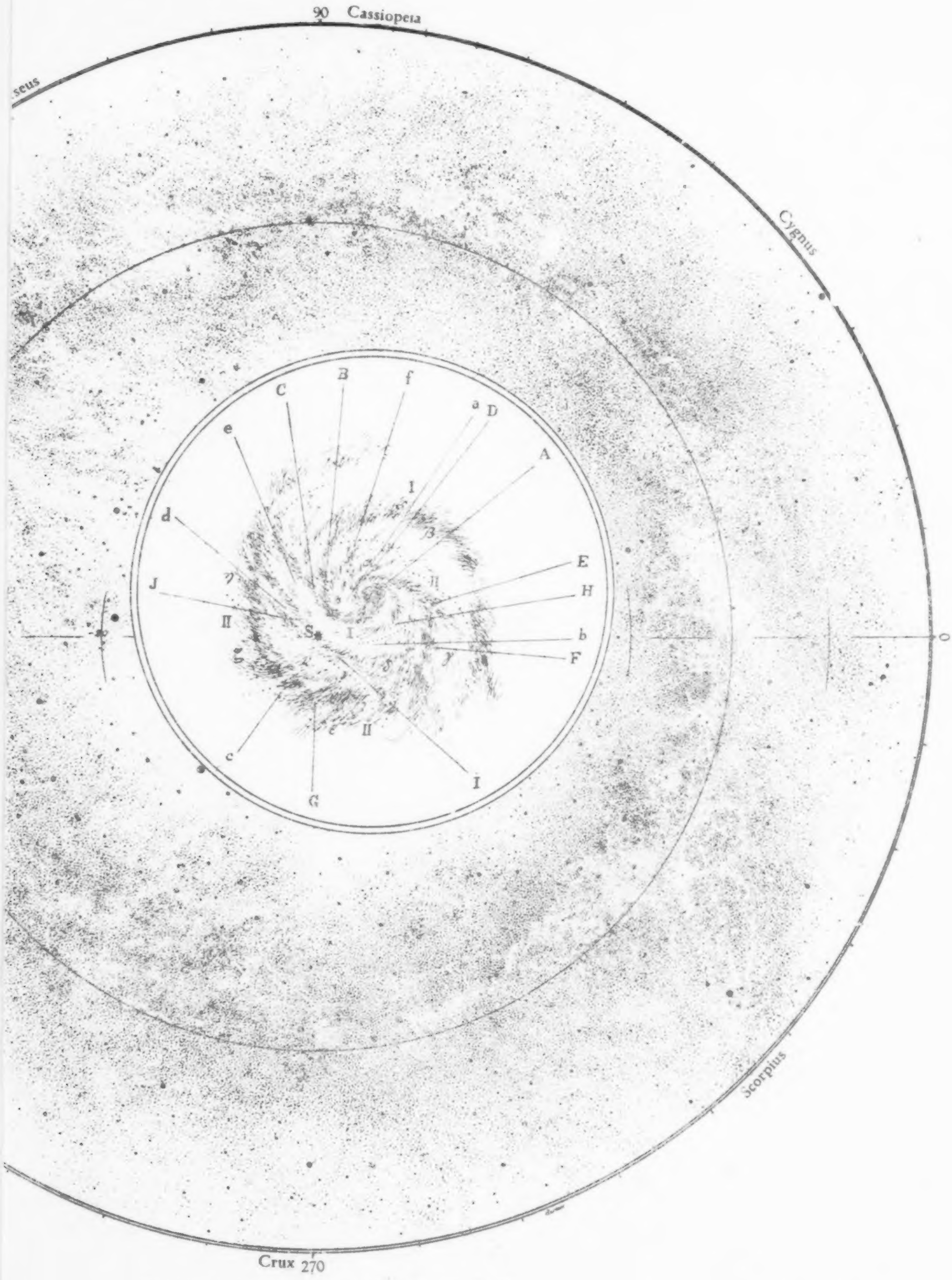
Dominion Observatory
Ottawa
September 24, 1912

PL



PHOTOGRAPHIC C

PLATE III



PHOTOGRAPHIC CHART OF THE MILKY WAY

A PHOTOGRAPHIC CHART OF THE MILKY WAY AND THE SPIRAL THEORY OF THE GALACTIC SYSTEM

By C. EASTON

A more reliable and more detailed representation of the apparent distribution of the stars than can possibly be furnished by naked-eye observations of the Milky Way is badly needed. I am well aware that the accompanying chart supplies this want only to a very limited extent. First of all, the aid of drawings and descriptions, resulting from visual observations only, cannot yet be dispensed with entirely in estimating the comparative brightness of distant parts of the Milky Way, no complete, perfectly homogeneous series of photographs embracing the whole zone being available; photographs and naked-eye drawings of the Milky Way, however, must picture somewhat different portions of the stellar world. Nevertheless the advantages of the camera above the human eye, for most purposes connected with the study of star-distribution, appealed so strongly to my mind that I preferred the somewhat vexatious labor of combining the available photographs of the Milky Way in a general chart, rather than trying to discriminate between the often conflicting representations given by different observers of the galactic zone, although I have myself spent some years upon a naked-eye study of the Milky Way. As an independent evidence, and for certain well-limited purposes, the drawings, and particularly the "isophotal drawings"[1] of the Milky Way, retain of course their value.

The advantages of photographs are, first, trustworthiness, second, accuracy, third, wealth of detail. On the other hand, it is a drawback that a limited portion only of each photograph—the central parts—can safely be used, and that extensive nebulosities, mingled with the stars, may impair the general traits of their distribution. In combining the photographs, the draughtsman must be careful to avoid those difficulties as best he can.

[1] Houzeau, *Uranométrie générale*, 1878; Easton, "Distribution de la lumière galactique," *Proc. Amsterdam Acad. Sci.*, 1903.

For some years I have been occupied in compiling materials[1] and training myself for the particular work of rendering an interpretation from photographs in a manner similar to that in which the visual observer interprets the corresponding parts of the sky. The result of this work was embodied in four sheets (not yet published) on the original scale furnished by Marth's galactic co-ordinates of the stars, about 17×10 inches.[2] In the accompanying chart, Plate III, constructed after these drawings, Marth's data have also been used. It shows the Milky Way between $+20°$ and $-20°$ galactic latitude. The circular projection renders a certain amount of distortion in the higher galactic latitudes inevitable, but this drawback is not serious since this is the only possible way to get an uninterrupted view of the whole galactic zone in the two hemispheres.

The photographs furnished each a certain amount of detail and the connecting of these details presented no great difficulties, overlapping photographs, often by different authors, being avail-

[1] A detailed list cannot be given here. I made use of the following photographs chiefly: Professor Barnard's "Lick photographs," published for the greater part in successive numbers of the *Astrophysical Journal*, *Popular Astronomy*, and *Knowledge*, since 1894; those of Professor Max Wolf, in *Knowledge*, since 1891, partly in *Die Milchstrasse*, Leipzig, 1908; Russell's photographs of the Southern Milky Way, Sydney, 1890; Pickering's "Harvard Series," covering the whole sky (prints of a number of these plates were prepared for me, in order to bring out the "cloud-forms," by Dr. Neuhauser, in Amsterdam); Bailey's "Southern Milky Way," *Harvard Annals*, **60**, 8, and **72**, 3; some photographs by Wilson, Mrs. Maunder, and others. Through the courtesy of Professor Kapteyn I was able to use a number of Professor Barnard's and Professor Pickering's photographs as early as 1906. I have also to thank the Leyden and Utrecht observatories for lending materials, and I am under great obligations to Professor Max Wolf, who kindly supplied me with a number of glass plates, among them two large-scale photographs especially taken for this purpose. Numerous photographs are scattered in different astronomical publications; the beautiful reproductions in the *Researches on the Evolution of the Stellar System*, **2**, by Professor T. J. J. See, deserving a special mention. Professor Barnard has just now completed a volume containing his "Lick photographs," his volume of "Mount Wilson plates," covering the entire galactic zone up to $-20°$ S.P.D., published by the Carnegie Institution of Washington, being on the way to completion. In most cases, of course, I preferred the use of glass plates to prints.

For a complete catalogue of photographic charts of the sky, up to the year 1908, see *Harvard Annals*, **60**, 9.

[2] These indispensable data were given by Mr. A. Marth in *Monthly Notices*, **53**, 78, 384, 420.

able in almost every case. In this manner the general picture of the Milky Way resulted with a minimum of bias on the part of the draughtsman. In comparing the brightness of widely separated regions it remained, however, necessary to resort to visual estimates, e.g., for the patches in *Sagittarius*, *Aquila*, and *Cygnus*.[1] Although I proceeded very critically, I must confess that this is the weakest part of the work, but it proved unavoidable, even the best photographic series not being sufficiently homogeneous to allow a direct comparison of such distant parts.

The contrast between the bright and dark patches and lanes has intentionally been somewhat exaggerated in drawing. The galactic equator, shown as a circle, was inserted after Marth's position of the galactic North Pole ($12^h\ 40^m + 30°$, 1880). I may, however, remark that the old determinations of the galactic plane, based on insufficient data, have but little value; the photographic chart furnishes a sound basis for a redetermination of the medial line of the galaxy.

The stars were inserted on the scale of their *visual* magnitudes.

Although great care has been taken to make the accompanying chart as complete and as reliable as possible, it will hardly be necessary to state that the author did not aim primarily at completeness and exactness of detail—for which the original photographs can be consulted—but to furnish a general representation of the galactic zone for orientation purposes, in the same way that small-scale maps are used in geography. We owe it to the great skill and the untiring perseverance of Barnard, Wolf, Pickering, Russell, Bailey, and other astronomers, that it is now possible to construct such a "photographic chart of the Milky Way."

There is a question as to whether we are justified in regarding this representation of the stellar distribution in the galaxy as something definite, in its *principal* features. I think we are.

It seems almost incredible that more sensitive plates and longer exposures, showing one or more magnitudes beyond the limits

[1] For the Southern Hemisphere: J. Herschel's description (*Outlines*, § 787); the *Uranometria Argentina;* Houzeau's "isophotal chart" in the *Uran. générale*, and T. W. Backhouse's valuable description in *Publ. of West Hendon House Observatory*, **2**.

attained in the best photographs now available, will give a very different picture of the Milky Way. Theoretical considerations and actual counts of stars[1] point to the probability that the bulk of the small stars, at least of those within the range of the existing instruments, belong to the already known agglomerations, and that they will not fill up the vacancies between these bright parts. The remarkable correspondence between the visual and the photographic Milky Way *in their most characteristic features* is also very significant in this respect. Compare, for instance, my description of the *Aquila-Sagitta* region of the Milky Way (*La Voie lactée*, 1893, pp. 38 f.) with Professor Barnard's photograph (*Astrophysical Journal*, **2**, 35, 1895). Besides, in comparing photographs taken with different lenses and different exposures, we find that not only the main features of the galactic picture are the same, but that very minute details are preserved throughout. A comparison of two photographs of the η *Argus* region may serve as an example. The one is by Russell, 1890 (*op. cit.*, Plate 1) with a lens of 6 in. aperture and 32 in. focus (Dallmeyer), exposure 3 hours; the other by Bailey, 1909 (*op. cit.*, Plate 4) with a Cooke lens of $1\frac{1}{2}$ in. aperture and 13 in. focus, exposure 13 hours. The latter photograph contains perhaps three times as many stars as the first, but virtually all the features, here so vividly depicted, are already indicated in Russell's photograph.

If further researches disclose outlying parts of the system, the present picture will yet hold good for the inner parts, considered as a whole.

I have insisted on this point, because it is evident that the photographic chart would be of little value if we could expect that better instruments and longer exposures would give us quite a different picture of the Milky Way. Happily the photographs of the galaxy, at least of that part of the system now accessible to our researches, may be likened to a sketch of a drawing or map, whose broad outlines remain unaltered as more detail is filled in; rather than to a misty landscape, which presents continually changing

[1] Newcomb, *The Stars*, chap. xx, concl. 4; Easton, *Astronomische Nachrichten*, Nos. 3270, 3803, *Astrophysical Journal*, **1**, 216, 1895.

views as the lifting fog discloses more and more distant houses and groves.

This picture, as we have it now before us, speaks for itself. It leaves next to nothing of the traditional simplicity of the galactic zone, as it is still described in textbooks: "a broad and ample road," vague and rather uniform, split in two over half its circumference. On the contrary, what strikes us most in our photographic chart is the sharp definition of many features, especially near the axis of the Milky Way, and the truly perplexing intricacy of structure, presented by the greater part of the zone. We even have a certain difficulty in recognizing the "two branches." It is true that a winding channel, composed of irregular vacancies and narrow lanes, frequently spanned by bridges of light, runs from about α *Cygni* along the galactic axis to κ *Circini*, where it is connected by a scarcely interrupted, serpentine lane with the "Coal Sack" in *Crux*.[1] This "great rift" (if we keep the name) can be traced on the other side as far as κ *Cassiopeiae* and even μ *Persei;* the remaining portion of the galactic zone presenting a somewhat different character.

Other striking features revealed by this chart are the long "lateral offsets," which often seem connected with groups of bright stars (as Boeddicker has already remarked), forming extensive lateral systems in *Taurus* and *Orion*, in *Cepheus*, and particularly in *Scorpius*. They were of course known beforehand, but the relations of these highly significant formations to the remainder of the zone were never so well exhibited. I would especially draw attention to the streams north and south of the galactic equator, converging to the bright central parts in *Auriga* and *Taurus*, a dark lane running between them, from the *Hyades* to β *Tauri* and θ and β *Aurigae* right across the Milky Way; then to the bright masses and streams encircling the "Northern Coal Sack," between α *Cygni* and α *Cephei*, and returning to the principal branch in *Cassiopeia;* also to a complicated structure, partly described by John Herschel (*Outlines*, § 789), in the constellations *Lupus*, *Scorpius*, and *Ophiuchus*. It is obvious that the extraordinary chasms

[1] Russell draws attention to these lanes, but does not state the connection of the rift with the "Coal Sack."

and bright and dark streams, discovered by Professor Barnard between 52 *Ophiuchi* and *Antares*, form but a part of a much more extensive system of interlacing branches—perhaps there are *two* systems present, seen in perspective and projected one against the other—covering the whole region to the north of the galactic axis between η *Serpentis* and β *Centauri*, and certainly related to the principal (southern) branch of the Milky Way, though separated from it by rifts and cavities. It can hardly be by mere chance that most of the streams radiating from the gigantic assemblage of suns about σ and ν *Scorpii* are directed to the very brilliant patches of milky light in *Scutum* and *Sagittarius*. It is obvious that all these formations are interdependent and not merely the outcome of a chance projection; the unity in this apparent complexity is highly significant. This whole region seems the counterpart of the great appendage in the northern Milky Way between β *Cygni* and γ *Ophiuchi*.

Space forbids drawing attention to many other characteristic features, revealed by the photographic chart; the reader may easily discover them for himself. I only wish to enumerate some general divisions of the galactic zone which seem indicated by the characteristics of different parts.

1. The brilliant parts β to γ and α to π *Cygni*, and π *Cygni* to κ *Cassiopeiae*, with the northern extensions over *Cygnus*, *Cepheus*, and *Cassiopeia*, perhaps as far as χ *Persei* and δ *Aurigae*.—The region β *Cygni* to γ *Ophiuchi* seems loosely attached to these parts.......................................*Alpha Region*

2. The southern parts between χ *Persei* and ϵ *Cygni* and as far as γ *Sagittae*.......................................*Beta Region*

3. The varied region in *Sagitta*, *Aquila*, *Scutum*, *Sagittarius*, *Ara*, *Norma*, as far as β *Circini* or α *Centauri*.—The series of bright patches at δ *Sagittae* and δ and ν *Aquilae* seems to hold an intermediate position between the "Alpha" and "Gamma" regions.......................................*Gamma Region*

4. The northern parts between η and ζ *Serpentis*, covering 58, ρ *Ophiuchi* and δ *Scorpii*, and stretching as far as ρ *Lupi* and α *Centauri*.......................................*Delta Region*

The characteristics of these four regions are easily recognized.

The remaining portion of the Milky Way, between *Crux* and *Perseus*, has many distinguishing characteristics: its light is relatively faint, it has no central rift, but extensive lateral offsets. It can be subdivided in three regions:

5. Roughly between *β Circini* and *ζ Puppis* (225°–285° galactic longitude) we come to a region with extensive ramifications: the somewhat fan-shaped divisions originating about *η Argus*. Already in *Centaurus* and *Crux*, the bright part, narrow and perforated in these constellations, "twists around," so to speak; between *Crux* and *η Argus* and farther on, the light decreases gradually, not from south to north, as in *Sagittarius* and *Norma*, but from north to south............................*Epsilon Region*

6. From 225° galactic longitude (about *a* and *b Mali*) onward, the curved wisps on the north side take another direction, and the southern extensions cease. Toward *Canis Major*, there is nothing left of a central rift, and the central parts, broad and faint, are almost structureless; but in *Puppis* and *Monoceros* we find a maze of wisps and narrow lanes, typical for this region, and noticeable even to the naked eye............................*Zeta Region*

7. The remaining part (165°–110° galactic longitude) is characterized by a rather brilliant though much furrowed central region, with loose and extensive streamers emanating from it, as ribs from a spine; there is a lack of continuity between this region and the outer parts of Alpha Region......................*Eta Region*

In the photographic chart of the Milky Way we now possess solid ground from which to test the spiral theory of the galactic system, enunciated in the *Astrophysical Journal*, **12**, 136, September 1900. Since the appearance of that paper, the spiral form, till then something exceptional in the known stellar world, has revealed itself, in the late Professor Keeler's words, "as the usual or normal accompaniment of contraction in cosmical masses," and the theory of Moulton and Chamberlin testifies that it may lead to important theoretical developments. The researches of Kapteyn, Kobold, Dyson, Eddington, Schwarzschild, and others render a closer inquiry into the fitness of the spiral theory as a working hypothesis highly desirable, as it has lately been shown[1] that the Milky

[1] J. C. Kapteyn, *Proc. Amsterdam Acad. Sci.*, January 1912.

Way is also of fundamental importance in relation to the star-streams.[1]

I am well aware that the great problem of the Milky Way can never be solved in this way, and that we may aim only at a plausible interpretation of known facts, and at a working hypothesis. I may be allowed to repeat what I wrote in the *Astrophysical Journal* twelve years ago: the figure in the center of our plate does not pretend to give even an approximate representation of the galactic system, but only to indicate in a general way how the stellar accumulations might be arranged so as to produce the phenomenon of the Milky Way—on the supposition of a spiral galaxy.

I have now taken as typical a figure intermediate between two of the best known spiral nebulae: *M 51 Canis Venaticum* and *M 101 Ursae Majoris*. The adopted spiral must answer to the condition that the principal features of the Milky Way result freely and plausibly from its projection on the sphere, viewed from the sun (*S*).

We must take into account:

1. The branches of the spiral are not situated in the same plane.[2] The first branch, stretching from the central nucleus outward, at first rises somewhat above the plane of the chart, but lies below it in its farther course. The second branch lies at first below, then comes nearer to the middle plane, crosses it somewhere in the direction *Se*, and continues to run above it for the greater part of its course. The central condensation itself would lie somewhat to the north of (above) the middle plane of the galaxy, as adopted by Marth (cf., however, what is said on p. 107).

2. Up to a certain distance from the sun, the stellar agglomerations must seem scattered, so that they cannot produce any milky light.

3. The apparent brightness of the patches and streams of the Milky Way depends not only on the richness of the stellar condensations and strata, but also on our point of view, in *S*, so that star-streams running almost exactly in our visual line must show as brilliant patches.

[1] Cf. T. J. J. See, *Researches on the Evolution of the Stellar Systems*, **2**, 1910, and Sv. Arrhenius, *Nord und Süd*, January 1912.

[2] Cf. the *Cassiopeia* to *Sagittarius* region of the photographic chart with a spiral nebula seen edgewise, e.g., "*H. V., 1 Ceti*," *Lick Publications*, **8**, Pl. 2.

I have indicated in the spiral figure the divisions α, β, γ . . . . corresponding to the "galactic regions" named above.

The central nucleus of the galactic spiral would project itself between β and γ *Cygni;* the northern continuation of this brilliant part, and the patch about α *Cygni*—a stream seen foreshortened in *D*—may be reckoned as belonging to the central condensation. The feeble interspiral streams to the left of the nucleus, together with the part of the first branch nearest to *S* (and therefore mostly resolved into stars), form the northern outlying parts in *Cepheus* and *Cassiopeia;* the apparent interlacing of these streams, as in *f*, producing the galactic condensations about ξ *Cygni;* they leave between them relatively dark spots, as the "Northern Coal Sack" indicated by *a*.

In the direction *SB*, the greater part of the spiral branch swings round (at κ and ι *Cassiopeiae*), bends somewhat to the south, and pursues its course through *Lacerta* and over ϵ *Cygni* ("Beta Region"). Some outlying streams in the direction *SC* form the branching structure between γ *Cassiopeiae* amd χ *Persei*, which terminates rather abruptly at η *Persei*. Here then ends the "Alpha Region," in this direction.

The second branch proceeds from the central nucleus mainly between β *Cygni* and *Sagitta*, being also in relation with the α *Cygni* stream. Descending in galactic longitude,[1] we find that the "knots" or "nodosities" of the branch in the direction of *F* must appear isolated, and the "fringes," curved away from those "knots," must be seen foreshortened—giving rise to the brilliant patches, somewhat in the shape of crescents or shields (*Scutum!*) from *Sagitta* up to the tail of *Scorpius* ("Gamma Region"). It is worthy of remark that "shield-forms" occur elsewhere in the Milky Way, the most remarkable instance being the series of bright patches between π *Cygni* and μ *Cephei* (Professor Barnard's photographs; see *Knowledge*, March 1894, and *Astrophysical Journal*, **21**, 48, 1905).

The spiral branch No. II (see chart) then approaches the sun,

[1] I will use the term "ascending" to indicate, along the galactic axis, the direction in which the galactic longitudes increase, viz., from *Aquila* to *Cassiopeia;* "descending" the opposite direction, viz., from *Aquila* to *Sagittarius*.

broadening and at the same time becoming vaguer and fainter ("Epsilon Region"). The bright stars are exceptionally numerous in those parts, as well as in the opposite direction (*SB* and *SA*), in accordance with the theory.[1]

Where shallow parts separate the "knots" in the convolutions, the lateral rifts, already present between the bright patches in *Sagittarius* and *Aquila*, become more evident. With increasing distance from the sun, beyond *Sc* (*descending*), the breadth of the central parts of the Milky Way again decreases. (The great width of the galactic zone in the regions *Scorpius* to *Sagittarius* and *Auriga* to *Taurus* is due to secondary streams, projected alongside the head-stream.)

We may look upon the "feeler" β *Cygni* to γ *Ophiuchi* as an appendage (*H*) of the central nucleus *A*; the series of patches from γ to ν *Aquilae* forming a side-branch (*E*) of the second convolution.

The interspiral region surrounding *S* is filled loosely with less condensed streams and strata of stars, whose general direction is tangential to the central nucleus, but which are crossed by others running between the great spiral branches and parallel to these. If those two main directions are everywhere present in the spiral system, as they show in the nebulae (cf. Ritchey's admirable photograph of *M 51*), they probably cause the intersecting streams in *Lupus* and elsewhere, and determine the outlines of the "blocks of light" about γ and μ *Sagittarii*, also of the "caves" near γ *Aquilae* and η *Argus*, etc. May not these two directions in the spiral strata be the expression of Kapteyn's two star-streams penetrating the whole stellar system?

The secondary streams between the branches of the spiral account readily for the aspect of the Milky Way in *Lupus*, *Scorpius*, and *Serpens* ("Delta Region"), and in the opposite direction ("Eta Region"); the bright and probably preponderating stars forming Gould's "belt," inclined at an angle of about 18° to the general plane of the galaxy. The "helium stars" are numerous in these parts. Beginning between *Cassiopeia* and *Perseus*, these

[1] Schiaparelli, "Sulla distribuzione . . . . ," *Publ. Brera*, **34**; Stratonoff, *Publ. Tachkent*, **1**; Easton, "Distribution de la lumière galactique," *Verhand. Amsterdam Acad. Sci.*, **8**, No. 3, p. 31.

strata run first below the general plane, crossing it in the neighborhood of the sun (that is why the points of intersection of Gould's belt with the galactic equator lie about at 90° and 270° of galactic longitude) to rise above the plane, in *Scorpius* and *Ophiuchus*, before uniting with the central parts of the system. Vistas as in *Sd*, between the extensive lateral streams in *Orion* and *Taurus*, are readily explained, and the faintest parts of the Milky Way as a whole, seen from *S*, must of course lie in the direction of *e* (*Perseus*) and *b* (η *Serpentis*).

We may perhaps limit our interpretation of the Milky Way to these remarks; the analogy could easily be pushed farther.[1] No geometrical postulates for the shapes of "stellar spirals" having been found as yet,[2] such an interpretation is always more or less arbitrary. To give an example: the *Scorpius* (Delta) Region might perhaps as well be placed outside as inside the great spiral convolution. My aim was to show that the spiral theory holds good in the face of the more reliable and more detailed picture of the Milky Way, furnished by the photographic method.

Two objections have been raised against this theory. The central nucleus in the nebulae seems much more important in comparison to the remainder than would be indicated by the importance of the bright patches in *Cygnus*. This is true, but in the photographs this preponderance of the nucleus diminishes as the excellence of the photograph increases; i.e., the nucleus itself is often unduly magnified, on a rather poor plate, by adhering portions of the convolutions. I have already insisted on the excessive brilliancy which certain distant portions of the spiral acquire, owing to their running almost exactly in our line of sight. For the rest, the proportion in mass between the nucleus and the branches of a spiral does not seem a serious criterion for a theory, if it is otherwise acceptable.

[1] It is noteworthy that Arrhenius (*Nord und Süd*, January 1912) arrives independently at a somewhat similar structure of the galactic spiral.

[2] Wolf, *Die Milchstrasse*, p. 19. Cf. See, *Evolution*, **2**, 39 f.; also E. v.d. Pahlen, *Astronomische Nachrichten*, No. 4503, and W. Sutherland, *Astrophysical Journal*, **34**, 3, 1911.

I can by no means share the opinion[1] that if the spiral theory is true, the many thousands of nebulae found by Keeler and Wolf must be "external galaxies," so that the confirmation or disproval of this supposition would be a test of the spiral theory. I wish only to remark that nobody will deny the existence of a whirlpool because he sees a number of small eddies in the convolutions of the great one. I think we may safely assume that the great majority of the small spiral nebulae, if not all, form part of our galactic system.

As to the part which those small nebulae play in the galactic system, I made an attempt, some years ago[2] to explain their relations to a spiral galaxy; contending in the first place that the usual idea of a quasi-symmetrical distribution of the nebulae in the two hemispheres, gathering around both galactic poles, is untenable; secondly, that (the *nubeculae* being excluded) the distribution of the nebulae shows maxima in galactic longitude 105° and 280°, about 90° from the supposed central nucleus in *Cygnus*. This peculiar distribution is readily explained if we may admit that our present observations only reveal to us, among those faint objects, those that are relatively near to us, and that the nebulae—perhaps remnants left behind by the rotatory movement of the system—chiefly fill up the space not occupied by the galactic convolutions, their distribution being complementary not to the distribution of the stars but to that of the "vast, irregular" nebulae.

On the other hand, the convolutions themselves seem to be extensively veiled by a stratum of nebulous light, which is perhaps nowhere wanting in the Milky Way proper, except in the "perforations," those singular holes and cracks discovered and studied by Barnard and Max Wolf. I see no evidence whatever of *continuous* masses of opaque matter lying between us and the stars, although such non-luminous matter must be scattered freely throughout the system. It is evident that the more or less luminous nebulosity is intimately connected with the stellar accumulations

[1] Ristenpart in Valentiner's *Handwörterbuch der Astronomie*, **4**, 123; cf. Newcomb-Engelmann, *Populäre Astronomie*, 3d ed., p. 618.

[2] Easton, "The Nebulae Considered in Relation to the Galactic System," *Proc. Amsterdam Acad. Sci.*, 1904, p. 189; *Astronomische Nachrichten*, No. 3969.

and strata of the Milky Way. Those clouds, composed of stars and nebulous matter exhibit in various degrees the influence of what W. Herschel already has indicated as the "clustering power," breaking up the coils of the spiral, as we see it in the nebulae *M 101 Ursae Majoris* and *M 33 Trianguli.*

In this "clustering" of the galactic clouds, the star-clusters apparently play an important part. Certain clusters have evidently been formed at the expense of the strata in which they are imbedded (see photographs of the bright patch in *Scutum*). In many other cases, they seem to act as centers for the grouping of the galactic condensations, and it is natural to think that these clusters have far more important masses than is indicated by their size and brightness.

The study of the photographs, together with the theoretical considerations indicated above, lead to a similar inference in regard to certain stars. In the galaxy there must exist a number of stars, immensely surpassing in mass the components of the galactic star-clouds—true miniature suns even in comparison to our own luminary. Those gigantic globes, especially where they are grouped together, as in *Scorpius*, seem to act a leading part in the dismembering of the galactic convolutions.

Certain parts of the galactic zone, probably analogous to the "knots" in the great spiral nebulae, exhibit forms in which the spiral structure, though less apparent than in the small nebulae, and (I think) in the system as a whole, is not altogether wanting. We sometimes come across very curious little spirals in the galactic regions—e.g., to the south of ϵ *Carinae*—but great masses also exhibit this peculiarity. So we find a spiral structure of 150 sq. degrees between α *Crucis* and ϕ *Argus*.[1] In this case, we may note *two* central nuclei, at κ and η *Argus*, so that the term *bihelical* seems more appropriate.

A comparison of the different types of spiral nebulae (*Lick Publications*, **8**, "Keeler Memorial") leads to the conclusion that our galactic system would seem to be already in an advanced stage

[1] Speaking of the bright region in *Scutum*, Professor Barnard wrote (*Astrophysical Journal*, **1**, 11, 1895): "I have often received the impression that this huge cloud of stars has been generated by some tremendous whirling motion."

of development, approaching rather to the *M 101* (*Lick*, Pl. 49), than to the *M 51* (*Lick*, Pl. 47) type.

It is natural to think that the Magellanic clouds should be analogous to the small nebula connected with Lord Rosse's spiral *M 51*. The analogy is strengthened by the resemblance in shape of the *nubeculae*, as seen in the well-known photographs of Russell, to the galactic formations about *η Argus* and elsewhere just described.[1] In any event, the actual connection with the main system, so obvious in the nebula, must be slight in the case of our galactic system. It is true that Backhouse[2] saw a wisp of galactic light uniting the Greater Magellanic Cloud with the Milky Way about *π Puppis*, and that, in some photographs, rows of stars seem to connect that *nubecula* with the galactic zone in *Carina*, but the evidence is doubtful. I must add, however, that Stratonoff's chart of the distribution of the nebulae shows a connection between the *nubeculae* and the *nebulae* in the southern hemisphere, and in my opinion the small nebulae form part of the galactic system.

AMSTERDAM
October 1912

[1] I believe that the great structural resemblance between the *κ* and *η Argus* region and the *Nubecula Major* has never been insisted upon. Russell's photographs Nos. 1 and 14, however, show it at a glance.

[2] Backhouse, *Publ. of West Hendon House Observatory, Sunderland*, **2**, 30.

ON THE OCCURRENCE OF THE ENHANCED LINES OF TITANIUM IN ELECTRIC FURNACE SPECTRA

By ARTHUR S. KING

In the course of an investigation now in progress on the variations in the spectrum of titanium given by different temperatures of the electric furnace, it seemed highly desirable to make a special effort to obtain the enhanced lines and fix their place, if possible, on a temperature scale. In general, enhanced lines are very difficult to produce in the furnace, as is to be expected from the fact that for most metals they appear in the arc only under special conditions, either close to the poles or in an interrupted arc which gives momentary conditions approaching those of the spark. However, the arc shows these lines for some substances much more easily than for others. Thus, the H and K lines of calcium, which are to be classed as enhanced lines, are also among the strongest arc lines. It was shown in a previous paper[1] that they may be obtained in the furnace at a stage considerably below the highest furnace temperatures, and exhibit a rapid rise in intensity with temperature. The enhanced lines of titanium also occur in the arc, some of them being fairly strong, though weaker than many of the arc lines. It has been shown in previous publications[2] that the furnace at high temperatures gives a spectrum for titanium comparing in richness with that of the arc, but the enhanced lines were notably lacking in the furnace. Their strength in the arc, however, gave promise that by forcing the furnace temperatures the enhanced lines could be made to appear. This has been accomplished, and some supplementary experiments have shown several remarkable phenomena bearing directly on the nature of enhanced lines in general which will be reported in this paper.

[1] *Contributions from the Mount Wilson Solar Observatory*, No. 32; *Astrophysical Journal*, **28**, 389, 1908.

[2] *Contributions from the Mount Wilson Solar Observatory*, No. 28; *Astrophysical Journal*, **28**, 300, 1908; *Contributions from the Mount Wilson Solar Observatory*, No. 60; *Astrophysical Journal*, **35**, 180, 1912.

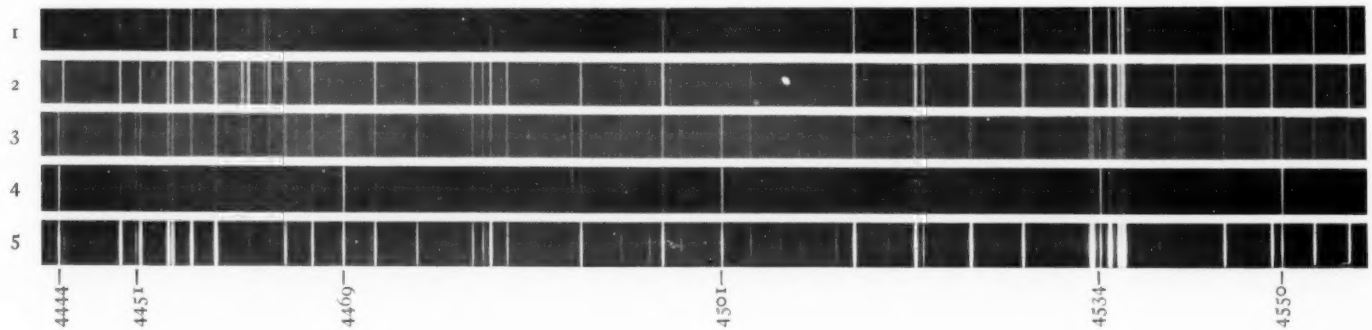
In the previous experiments very high temperatures were occasionally employed, but with only moderate dispersion, so that the continuous spectrum given by such a temperature was strong enough to conceal faint lines. An increase of scale was required, cômbined with brightness of the spectrum. Fortunately, a new plane grating was available, ruled by Anderson on the Rowland machine, which gave very bright spectra in the first and second orders in spite of its small area (6.3×7.2 cm). The second order of this grating was used in the Littrow spectrograph of 30 ft. (9.1 m) focal length, the scale being approximately 0.9 Å. per mm. The photographs were made on Cramer "Crown" plates for the region from λ 4220 to λ 4600, in which range titanium has about forty enhanced lines of varying intensities, but all showing a behavior in arc and spark quite typical of this class.

The tube resistance furnace was used with its parts arranged as described in previous papers, but without any jacketing material around the tube. The chamber was pumped out to a low pressure. The tubes of Acheson graphite first employed were 12.5 mm inside diameter, 18.3 mm outside diameter, and 30.5 cm long, the heated portion being 20.3 cm long. A potential of 30 volts on these tubes gave 1600 amperes, falling to about 1500. Pyrometer readings for several trials gave above 2600° C, when the instrument was sighted at the interior of the tube. This value is probably low, being measured for an open tube. Under such conditions, all of the stronger enhanced lines appeared very distinctly for exposures lasting 3 minutes. A 2-minute photograph, showing less continuous ground, is reproduced as No. 2 of Plate IV. The enhanced lines are plainly visible in the negative, but can hardly be expected to show in the reproduction. No. 1 in this plate is the spectrum for about 2400° and shows no enhanced lines.

We thus have the result that the temperatures attainable in the furnace, when operated in the regular way, will give all classes of titanium lines, and so far as the examination of the visible spectrum has proceeded, it appears that all lines of titanium appearing distinctly in the arc can be obtained in the furnace.

The next step was an attempt to obtain still higher temperature by the use of thinner tubes. Tubes with slightly thicker walls

PLATE IV



Titanium Spectra Given by Electric Furnace and Arc, Showing Development of Enhanced Lines when Tube Burns Through

1. Regular furnace spectrum for about 2400° C
2. Regular furnace spectrum for about 2600° C
3. Spectrum when slit was covered just after tube burned through
4. Spectrum given chiefly by conditions following break of tube
5. Spectrum of carbon arc containing titanium, taken on same plate as 4. The stronger enhanced lines are marked

than those previously used were turned down so as to give a wall about 2 mm thick for 5 cm at the middle of the tube, the powdered titanium carbide being placed in the middle of the thin portion. As surprising effects were soon obtained with these tubes, the experiments will be described in some detail.

A preliminary heating for 1 minute at about 1000 amperes drove off the more volatile substances, but did not melt the titanium. The tube would stand 1400 amperes at 25 volts for several minutes, giving a rich titanium spectrum with the enhanced lines visible. When 30 volts were placed on the tube, the current went above 1600 amperes (the limit of the ammeter scale), remained there a few seconds and began to drop, while the appearance of the image of the tube's interior projected on the slit showed that a violent vaporization of carbon was taking place. When below 1500 amperes the current fell rapidly, showing that the tube was burning through, with the formation of an arc. This arc often held for 5 seconds or more with the ammeter usually registering about 800 amperes, before the ends of the tube were burned so far apart that the arc broke. During this time a voltmeter connected across the furnace terminals remained close to 30 volts, though increasing 2 or 3 volts at the beginning of the arcing stage. With a gradual burning around the circumference of the tube before the formation of the arc, it is doubtful if a high momentary voltage occurred such as would result from a sudden complete break. The voltmeter would not have recorded this and tests have shown that the spectra obtained were not materially influenced thereby.

When the furnace was opened after cooling, the tube was found burned apart near the middle of the thin portion, a space of 3 to 4 mm separating the edges all around the circumference. (Contraction of the parts during cooling may have altered the original interval.) Each section of the broken tube thinned down gradually at each side of the break, beginning about 1 cm from the sharp edge, showing how the carbon had vaporized with decreasing violence at each side of the weak point where the break occurred. An interesting feature was that this vaporization took place almost entirely within the tube, the outside diameter remaining the same up to

the break. This was doubtless due to the strong radiation from the exterior of the tube, no jacket being used.

By making a large number of runs of the furnace with tubes of the same kind, it has been possible to photograph the spectrum given at various stages of the run which, from the time of turning on the 30-volt current to the final breaking of the arc, never lasted more than 30 seconds. Three plates were made for the conditions prior to the breaking of the tube, when it was wearing thin with strong vaporization of the carbon. The slit was covered as nearly as possible just before the instant of break. These plates were underexposed, but the enhanced lines, though faint, were relatively strong in reference to the arc lines as compared to photographs taken under the regular furnace conditions with a long tube of uniform thickness. This evidence indicates a slight relative strengthening of the enhanced lines with rising temperature while furnace conditions may be considered still to exist.

A photograph for which the slit was covered just after the tube burned through is shown as No. 3 of Plate IV. This registers for the most part the conditions preceding the break, but in the short interval in which the plate was exposed during the burning of the tube the enhanced lines appeared so strongly that the integrated effect gives them about the same intensity relatively to the arc lines that they usually have in the carbon arc containing titanium. Number 4 of Plate IV represents a stage more favorable to the enhanced lines. The tube broke sooner than usual and the plate was exposed after the break longer than in the case of No. 3. The enhanced lines are seen to predominate strongly in the spectrum. The relative intensities resemble those of the condensed spark, but the arc lines show the peculiar softness characteristic of furnace lines. Spectrum No. 5 is that given by the carbon arc containing titanium, from which the usual strength of the enhanced lines in the arc may be seen.

It is evident from the way the enhanced lines stand out in No. 4 of Plate IV that conditions during and after the burning through of the tube are especially favorable for the production of these lines. A consideration of what transpires during this part of the experiment shows that we have here a source different in

many respects from the furnace, arc, or spark as regularly operated, and perhaps approaching nearer to the conditions of radiation in solar and stellar atmospheres than is done in any of our laboratory sources. An important point is the high concentration of electrical energy over a very short stretch of the tube. Just before the break the transformer supplies from 40 to 50 kilowatts, a very large part of which must be used over the 5 cm of thin tube, the rest of the circuit being made up of low-resistance connections and the relatively thick tube at each side of the turned-down portion. When the tube has burned very thin at the weakest portion, the rupture must be in the nature of an explosion, in which probably the highest temperature is generated that can be obtained from carbon at the existing pressure. When the tube has burned through around its circumference, the vapor inside whose light falls upon the slit may be considered as surrounded by a wall of arc which must consume nearly all of the 24 kilowatts which the transformer usually supplied for a period of from 5 to 15 seconds. When we consider that the power ordinarily used for an arc in the laboratory is less than 500 watts, the energy consumed in the two sources is seen to be of quite a different order.

While the conditions following the break of the tube are widely different from the regular furnace conditions, the low potential employed furnishes an important difference from the arc or spark. As has been noted, the voltage across the terminals outside the furnace appeared not to rise above 33 volts. Granting the possibility of a high momentary potential which the voltmeter would not follow, experiments have shown definitely that the conditions favorable to the enhanced lines extend through the arcing period and are not limited to the instant of explosion. The most decisive test on this point was made by taking two successive photographs, the exposure for one being made as the tube burned apart, that for the other beginning about 10 seconds later. The second photograph showed all of the enhanced lines in that region, and very little besides.

The foregoing experiments had been made with an image of the interior of the tube about 2 cm in diameter projected on the spectrograph head, the light which passed through the slit (4 mm long)

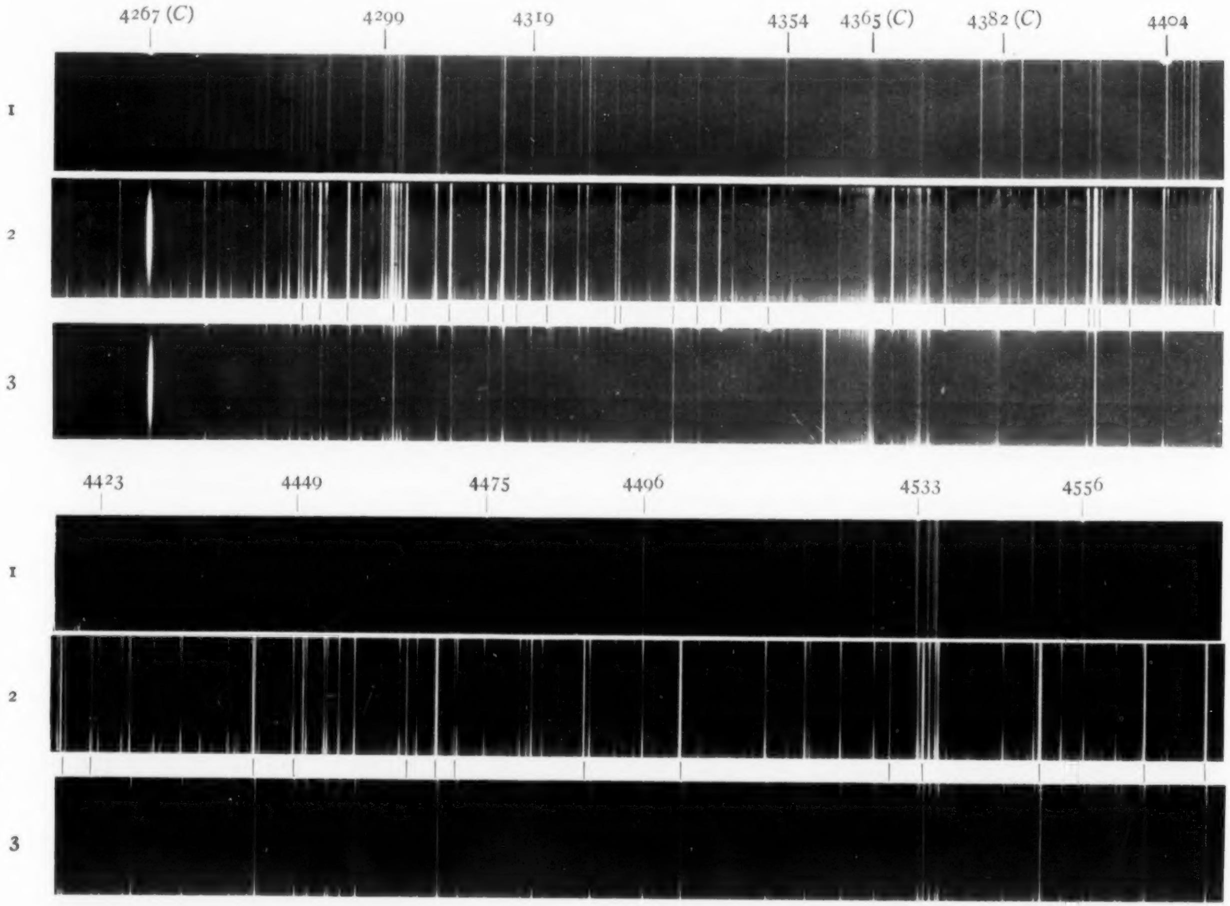
being taken from the central part of the image. It now occurred to me that there might be a distinct difference between the spectra given by the vapor at the center and by that near the wall where the arc was in action. As the spectrograph shows almost no astigmatism, it was a simple matter to test this. The length of slit used was increased so as to pass across the diameter of the image from wall to wall. When a tube was used at a temperature which did not burn it through, there was no perceptible difference in strength or appearance of the lines from center to wall, so that in the regular operation of the furnace the long slit offers no advantage.

For the conditions when the tube was burned through, however, a difference of the most striking nature appeared. The non-enhanced lines were much weaker in the center of the tube than near the wall, *while the titanium enhanced lines showed almost no change in intensity across the diameter of the tube.* In other words, the enhanced lines were very strong in the middle of the tube relatively to the lines characteristic of the arc. This phenomenon was photographed on six plates with no variation in the general features.[1] The spectra in Plate V were made with the long slit as described, and are reproduced almost full scale. Each spectrum is in two sections. No. 1 is a true furnace spectrum taken with a turned-down tube but with only 25 volts, which did not burn it through in the minute required for the photograph. Nos. 2 and 3 were made by exposing at the moment of break with 30 volts on the tube. The enhanced lines are marked in No. 2. Spectrum No. 3 has less general intensity and the enhanced lines stand out in the middle of the strip more distinctly than in No. 2. The uniformity of intensity across the diameter of the tube makes the selection of the enhanced lines a matter of the greatest ease, and is striking evidence of their dependence on a different physical condition of the vapor than that favorable to the non-enhanced lines and to the carbon flutings,

[1] Note added January 1913:

Supplementary photographs have been taken extending from λ 3900 to λ 5227, in which all of the titanium enhanced lines in this range have been obtained in the broken-tube spectrum. Their intensities relative to the arc lines were found to be fully in accord with those of the lines discussed in this paper.

PLATE V



Spectra of the Electric Furnace Photographed with Long Slit, Showing High Relative Intensity of Enhanced Lines in Center of Tube when Tube Burns Through

Scale: 1 mm = 1 Å

1. Regular furnace spectrum with unbroken tube

2. and 3. Spectrum given after tube burns through. Titanium enhanced lines are marked on margin of 2

of which the band with heads at $\lambda\lambda$ 4382, 4371, and 4365 comes out strongly. At λ 4267 appears the spark line of carbon, strong in the center and scarcely visible at the wall of the tube, its behavior being just opposite to that of the carbon bands. This line will be discussed farther on.

The enhanced lines would thus seem to show a distinct lack of dependence upon either temperature difference or potential fall. A temperature difference as we pass from the arcing wall of the tube toward the center is to be expected, and this is borne out by the diminution in intensity of the non-enhanced lines. The carbon flutings also, whose intensity in the regular furnace closely follows the temperature, are much weaker in the center of the tube. Likewise, such potential fall as there is in this low-voltage arc must be much greater close to the wall than at a point in the center of the tube 6 mm distant from the arcing point. If a decided difference in vapor density prevails between center and wall of the tube, the enhanced lines might be less affected by this than the arc lines. It is difficult to say how great a difference of this sort there may be, but the width of the arc lines, which is governed largely by the vapor density, seems to decrease less rapidly toward the center of the tube than does their brightness, which is controlled, for this class of lines, chiefly by the temperature.

The spark line of carbon, λ 4267, shown on Plate V, appeared on all plates taken with the burned-through tube, being strong and unsymmetrically reversed in the center of the tube, and decreasing in strength toward the wall, until it was almost invisible at the edge of the image representing the point nearest to the hot carbon. As has been noted, the vaporization of the carbon was very violent under the conditions of this experiment, but the line does not appear in the furnace when the tube does not break nor in the carbon arc burning with continuous current. Crew and Spence[1] obtained it in an arc in which one terminal was a rotating disc of graphite. They describe its behavior as follows:

> A very curious thing happens with this rotating graphite arc, viz., the appearance of the well-known carbon spark line at λ 4267, a *Hauptlinie* of Eder and Valenta. Few lines are so sensitive to rapid changes in E.M.F.

[1] *Astrophysical Journal*, **22**, 199, 1905.

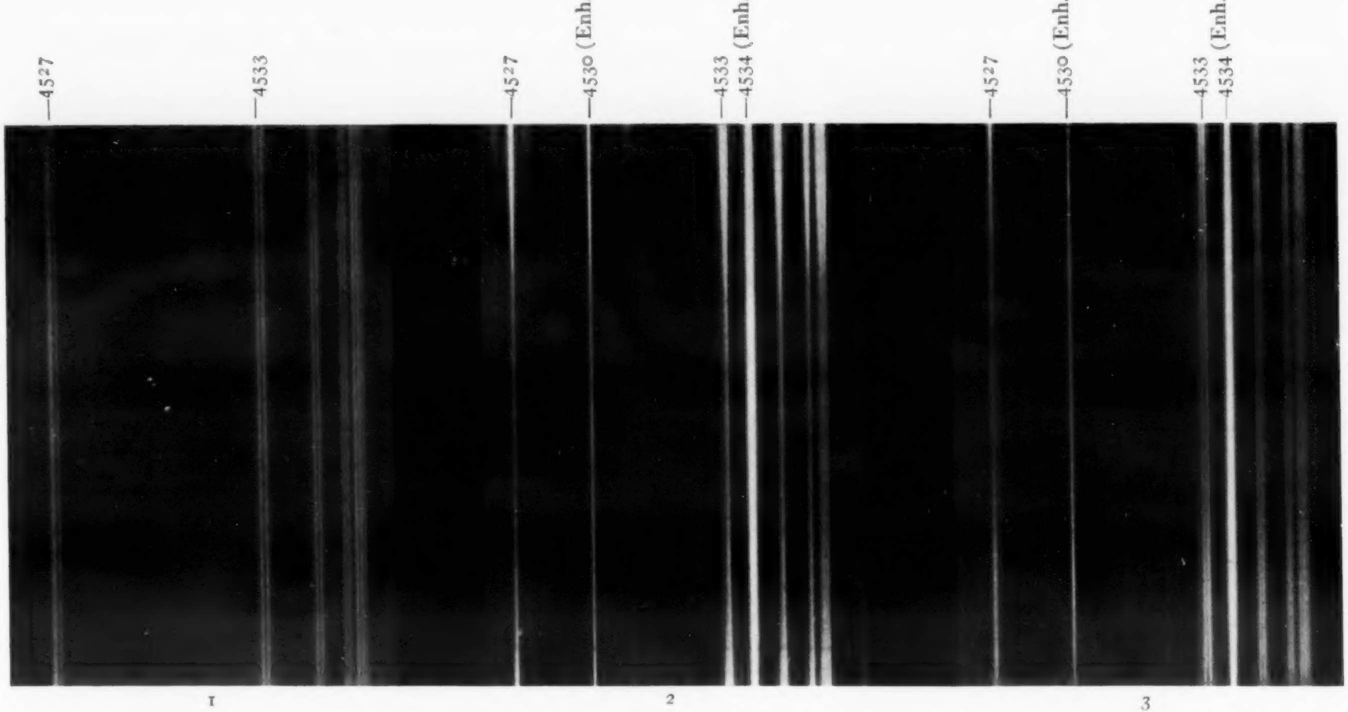
This line is strong in the spark between amorphous carbon poles, *provided they are cold;* if, however, the carbon tips have been heated, the line disappears even from a powerful transformer spark. On the other hand, this line which does not appear in the ordinary carbon arc comes out strong in the rotating graphite arc, where the natural quickness of break which characterizes the graphite arc is accentuated by the motion of the electrode. In other words, this line disappears with great readiness from its natural source (the carbon spark) and appears, on slight provocation, in a source where it might be least expected, the graphite arc. Such capricious behavior might lead one to hesitate in assigning this line to carbon; but its presence in pure Acheson graphite leaves little room for doubt as to its identity.

It is characteristic of enhanced lines in general that they weaken in the spark when the electrodes become hot; also, that such lines in many cases are given by the rotating arc used by Crew,[1] especially when operated in an atmosphere of hydrogen. The decrease of potential gradient in the spark with hot electrodes and the increase due to the sudden break in the rotating arc have been given as the most plausible explanation of this behavior of the enhanced lines. An extended investigation may prove that this conduct of the carbon line λ 4267 is typical of those enhanced lines which do not appear at all in the arc when burning continuously, such lines being a step more difficult of production than the titanium enhanced lines. When we have a source of great energy like the burned tube which is able to produce lines of all classes, the present evidence indicates that the arc lines, the titanium enhanced lines, and such lines as λ 4267 form a sequence for which a high potential gradient is not necessary and the region of highest temperature becomes increasingly unfavorable for the successive classes. The proximity of hot carbon appears as destructive to the carbon spark line in the furnace as in the spark while as we recede from the walls the vapor rapidly becomes better able to give this line.

Another highly interesting phenomenon appears in the spectrum when the tube burns through. The ends of the long lines, given by the vapor close to the arcing walls, show nearly uniform brightness across their width, while as we approach the middle, the red side of the line becomes constantly stronger. If the line is reversed, the dissymmetry of the two sides is very marked; if unreversed,

[1] *Astrophysical Journal*, **12**, 167, 1900.

PLATE VI



Spectra Taken with Long Slit before and after the Break of Tube in Electric Furnace, Showing Dissymmetry of Lines after Break, together with Appearance of Enhanced Lines

1. Regular furnace spectrum with unbroken tube
2. Spectrum when plate was exposed at moment of break
3. Spectrum combining effects of 1 and 2, exposure covering period before and after break of tube

chiefly the red side remains at the middle. This difference does not show for the titanium enhanced lines, though these lines being narrow and "hard" could not be expected to show it so distinctly if it is present. The spark line of carbon is very unsymmetrically reversed (see Plate V), the red side being almost twice as strong as the violet.

The effect, as compared to the regular appearance of the furnace line, may be seen in Plate VI. Three spectra are shown, taken with the same sort of tubes and the same optical arrangements, the conditions for the third being a combination of those for the first two. No. 1 is a regular furnace spectrum exposed 1 minute. The lines show little difference in intensity from end to end and are symmetrical in structure, whether reversed or not. Traces of the enhanced lines are barely visible in the negative. No. 2 is the broken-tube spectrum taken in 15 seconds, the plate being exposed at the moment of break. The large scale shows to advantage the variable intensity of the arc lines from end to end as compared with the unchanged condition of the enhanced lines, but it is given chiefly to illustrate the difference in structure at center and ends of the arc lines. The reversed lines show almost complete symmetry when given by the vapor near the wall of the tube, while in the middle of the tube little remains of the violet side of the line. The unreversed line $\lambda 4527$ has the same structure narrowed down. No. 3 was made by an exposure of about 1 minute before the break, followed by 12 seconds while the tube was burning through and arcing. The superposition of the two states is seen in the presence of strong enhanced lines, while the arc lines are nearly in the state of No. 1, but show a weakness and dissymmetry in the middle caused by the short period corresponding to No. 2.

These photographs seem to offer unmistakable evidence of a difference in condition between center and wall of the tube which affects the structure of the line. Whether it is a real shift of the maximum or an unsymmetrical widening cannot now be decided. It is probably the latter, but in any case it is a disturbance which would greatly affect wave-length measurements and must be recognized as a condition likely to occur in any source in which the vapor approaches a state such as we have here. As would

be expected, different photographs do not show this effect in the same degree. The general condition is always the same, but the violence of the rupture of the tube and resulting arc will not be alike in two experiments. A variability of the excitation during the time of an exposure is evident from the poor definition of the lines given by the broken tube, indicating the integration of a changing state; while the regular furnace lines, if the temperature is kept constant, are excellent for measurement. This difference in the quality of the lines is plainly seen in Nos. 1 and 2 of Plate VI.

The connection of this effect with another spectroscopic phenomenon is of special interest. It is well known that the condensed spark gives lines whose wave-lengths in many cases measure greater than in the arc.[1] Whether this is a real displacement or not, measurements are affected to such an extent that lines given by a strongly condensed spark are useless for accurate standards. The conditions in the middle of the furnace tube, when broken, give lines of such relative intensities as are obtained in the laboratory only with very powerful sparks, and we have the same apparent displacement toward the red as in the spark, when referred to the end of the line which is produced by vapor apparently more nearly in the state of the arc. This difference in the case of the furnace spectrum can scarcely be ascribed to pressure, a hypothesis for the spark which has been carried to the extent of estimating the pressure of the spark from known values for pressure displacements.

These preliminary observations appear very promising in the way of furnishing supplementary data as to the conditions for producing the enhanced lines, our knowledge of which has advanced slowly owing to the more or less conflicting evidence given by the arc and spark phenomena. Among the astrophysical applications, which can be made when observations for a number of elements are available, one of the most important will probably be to the spectrum of the chromosphere. The presence of enhanced lines in the upper regions of the sun's atmosphere agrees with the furnace results in that these lines may appear in regions where the highest temperatures are not to be expected.

[1] See, among others, N. A. Kent, *Astrophysical Journal*, **22**, 182, 1905.

SUMMARY

The leading features brought out in this study may be summarized as follows:

1. The enhanced lines of titanium appear in the regular furnace spectrum for temperatures probably somewhat higher than 2600° C; but are very faint compared to the arc lines.

2. At still higher temperatures, while furnace conditions still exist, there are indications of a slight increase in the relative strength of the enhanced lines.

3. When the furnace tube burns through with the formation of a low-voltage arc, the consumption of electrical energy at the point being very large, the enhanced lines of titanium and the spark line λ 4267 of carbon appear with an intensity usually attainable only in powerful sparks.

4. By photographing with the slit across the image of the tube's interior, the relative strength of the enhanced lines is seen to be much greater in the center of the tube than near the wall, this effect being very pronounced in the case of the carbon spark line.

5. The vapor in the center of the broken tube shows a tendency to give a line farther to the red than that near the wall, this being shown in the increasing dissymmetry of the lines from the end toward the middle. The effect is in harmony with the action of the condensed spark.

Mount Wilson Solar Observatory
December 4, 1912

MEASUREMENTS OF SOLAR RADIATION[1]

By C. G. ABBOT

Readers of this *Journal* are informed in two articles by Mr. F. W. Very:[2]

(1) The Violle actinometer [is] an instrument of unsurpassed precision.

(2) The absorption of solar radiation by the earth's atmosphere not only varies with the changing composition of the atmosphere from summer to winter, but is inconstant even during a single day of cloudless sky.

(3) The records of the Crova actinograph show that the chance fluctuations have their greatest magnitude in the middle of the day.

(4) The great fact of this midday atmospheric quality is so obtrusive that it cannot be ignored, and the Bouguer formula must be modified in order to cover the actual facts.

(5) [Referring to the diurnal march of the intensity of the solar radiation:] The portion of the diurnal curve between the limits of four and ten atmospheres conforms tolerably well to the conditions needed for a determination of its slope and general form, and, as a rule, it would seem to be the best part of the curve to select for computation.

(6) [Referring to some high-level measurements which he gives in the first paper:] A smooth curve passed through these points (Fig. 1) meets the axis of X at 3.5 calories per sq. cm. per min., which is the solar constant of radiation.

(7) I obtain . . . . from observations by Savélief, ... 3.606; from observations by Kimball, ... 3.698 in calories per sq. cm. per minute.

(8) My determination of the solar constant from the sounding-balloon measurement of M. Violle, described in my paper in the preceding article, is in good agreement with Savélief's observations. By the treatment recommended here, Kimball's observations are also brought into accord, not only without doing violence to the data, but by the application of precautions which are obviously required.

If these things are so, we must infer: first, that the work of Michelson, Ångström, Callendar, Marvin, Abbot, and Aldrich in the devising, perfecting, and testing of pyrheliometers has led to no improvements over the instruments which existed forty years ago; and second, that the work of the Smithsonian Astrophysical

[1] Published by permission of the Secretary of the Smithsonian Institution.

[2] *Astrophysical Journal*, **37**, 25–47, 1913.

Observatory since 1902 has been principally misguided and worthless. I therefore find myself directly interested in these statements, although no mention of me or of my work has been made by Mr. Very directly in connection with them.

I. PYRHELIOMETRY

I quote the following extract from a recent paper by Abbot and Aldrich:[1]

Summary.—A new form of standard pyrheliometer has been devised and tested. In this new instrument, as in the water-flow pyrheliometers, the solar rays are absorbed in a deep chamber approximating to the perfect absorber or "black body." Means are provided for introducing electrically test quantities of heat.

It is shown that with Standard Water-flow Pyrheliometers Nos. 2 and 3, and the new Water-stir Pyrheliometer No. 4, test quantities of heat may be measured to within 1 per cent.

A summary is given of all definitive comparisons of the three standards just named with Secondary Silver-disk Pyrheliometers, and also the net of inter-comparisons connecting all Smithsonian secondary pyrheliometers now in use. From these data are derived the best values of the constants of all these secondary pyrheliometers. This system of pyrheliometry we call "Smithsonian Revised Pyrheliometry of 1913."

It rests on 72 comparisons on 20 different days of 3 different years with 3 standard pyrheliometers of different dimensions and 2 widely different principles of measurement, all capable of recovering and measuring within 1 per cent test quantities of heat, and all closely approximating to the "absolutely black body." The 72 comparisons, 40 at Washington, 32 at Mount Wilson, were made in 6 groups. The maximum divergence of the mean results of these groups is 1 per cent. Hence it is believed that the mean result of all the comparisons made under such diverse circumstances must be within 0.5 per cent of the truth. The probable error is 0.1 per cent. It is believed that this standard scale is reproducible by the secondary pyrheliometers with the adopted constants given to within 0.5 per cent. The divergence of this scale from that of Ångström appears to be 3.9 per cent.

Experiments as yet unpublished have shown that an increase of at least 2 per cent should be applied to the readings of the Ångström instruments, so that the agreement between the Ångström and Smithsonian scales of radiation would then become practically exact. It is also known that the ice calorimeter pyrheliometer of

[1] *Smithsonian Miscellaneous Collections*, **60**, No. 18, p. 7, 1913.

Michelson, the new electrical compensation pyrheliometer of Callendar, and the new electrical disk pyrheliometer of Marvin are in very close accord with these scales of radiation.[1]

Two of the three applications of the Violle actinometer referred to by Mr. Very occurred on the expedition to Mount Whitney of the late Dr. S. P. Langley in 1881. Referring to the *Report of the Mount Whitney Expedition*, we learn the following facts in regard to the use of the actinometers in question.

Three Violle actinometers were carried on the expedition, and of these No. 1 and No. 3 were read at Mountain Camp or on the summit of Mount Whitney.[2] The determination of the water equivalents of the sun thermometers of the instruments is given on pp. 78 to 86 of the report. From this we find that the water equivalent of No. 1 was about one-fourth gram and of No. 3 about one-half gram; that the adopted value for No. 1 differed by 4 per cent from a check value found by other means; the adopted value of No. 2 differed by 6 per cent from its check value; and for No. 3 the difference was very slight. However, the thermometer No. 3 was broken in the transit, so that the determination of its constant was made by other methods than those of the other two instruments. In each case an allowance of 8 per cent was made for the conduction of heat along the glass stem of the thermometer. Pp. 100–115 of the *Report* are given over to the discussion and determination of six corrections to be applied to the readings of the instruments. The sum total of these corrections amounted to 25 per cent and 23 per cent for high and low sun, respectively, at Lone Pine; 23 per cent and 21 per cent for high and low sun, respectively, at Mountain Camp. To these corrections Mr. Very has now added a few others, so that, including the correction for loss of heat by conduction along the stem of the thermometer, we may say that about ten different corrections are applied, making a total increase of the reading of the actinometers of about 35 per cent.

[1] See *Transactions International Solar Union*, **2**, 175, 1907; also *Proceedings Royal Society London*, Series A, **77**, 6, 1906; also *Bulletin Mount Weather Observatory*, **3**, Part 2, p. 84.

[2] The results obtained with No. 1 differed from those obtained with No. 3, and had to be reduced to the scale of No. 3. See *Report*, p. 98.

I should regret exceedingly to make any criticism of the apparatus or the work of my esteemed friend Professor Violle; but in this case I feel less hesitancy because, so far as I know, neither the magnitude of these corrections, nor in many cases even the necessity for them, has been passed upon or approved by him.

We may now inquire whether it is probable that these corrections were necessary and exact. In order to do so it will be convenient to consider the observations at highest sun made on ten different days during the expeditions of 1908, 1909, and 1910 of the Smithsonian Astrophysical Observatory to the summit of Mount Whitney. The values which are given below are the highest observations made during each of the ten days; and, *as it happens, they are also the observations made at the least zenith distance of the sun for each of the days*, a fact of interest in connection with some of Mr. Very's statements.[1]

Station: Mount Whitney, California. Altitude: 4420 meters
Observer: C. G. Abbot

Date		Hour Angle	Secant z	Pressure Aqueous Vapor	Precipitable Water*	State of Sky	Calories per cm.² per Min.
1908	Aug. 24...	2^h51^m W.	1.445	1.24mm.	.....	Cloudless	1.594
	Aug. 25...	1 45 E.	1.250	0.92	.....	Cloudless	1.663
1909	Sept. 2...	3 28 W.	1.750	1.80	.....	Cloudless	1.561
	Sept. 3...	0 54 E.	1.175	1.91	.....	Exceptionally good	1.691
1910	Aug. 12...	1 14 E.	1.123	2.37	1.07mm.	Cloudless	1.635
	Aug. 13...	0 50 E.	1.101	2.34	0.85	Cloudless	1.622
	Aug. 14...	1 28 E.	1.150	1.98	0.69	Cloudless	1.643
	Aug. 15...	0 8 W.	1.080	2.00	0.65	Cloudless	1.644
	Aug. 16...	0 12 E.	1.085	2(.48)	.....	Cloudless	1.617
	Aug. 17...	0 2 E.	1.080	1.30	.....	Exceptionally good	1.694

* "Precipitable water" means the depth of liquid water which would have been produced if all the water-vapor in the path of the beam between the pyrheliometer and the sun had been condensed. Determined from bolographic observations by F. E. Fowle. For method see *Astrophysical Journal*, **35**, 149–162, 1912.

If there was anything in the higher atmosphere which tended to produce a haze or undue diminution of the intensity of the solar radiation at Mount Whitney on September 3, 1909, and

[1] In all Smithsonian solar constant work pyrheliometer readings are made not only at high and low sun, but at many intermediate zenith distances besides, so as to enable us to form a judgment of the uniformity of the sky conditions which have prevailed.

August 3, 1910, it was certainly absolutely invisible to the eye, and non-apparent with the apparatus which I had at my command. I remember repeatedly holding up a single finger at such a distance as barely to cover the sun's disk. The sky then appeared to have a deep blue color right up to the very edge of the sun. There was no perceptible whiteness at all. The quantity of aqueous vapor present was, as the measurements show, extremely slight, so that less than 1 mm. of precipitable water intervened in the path of the solar rays between the spectro-bolometer and the outer limit of the atmosphere. I see no reason to believe that the intensity of the radiation of the sun at these times was appreciably less than it was at the time of the measurements made by Keeler and by Nanry in September 1881, to which Mr. Very refers. Hence I feel convinced that we must attribute the difference between 1.70 calories, the highest value which I found, and the practically 2 calories found by Mr. Very, to a difference in pyrheliometry.

In view of the agreement of different kinds of pyrheliometers which I have cited above, I think it is more reasonable to conclude that the measurements made during the year 1881 were too high than that the measurements made during the years 1908 to 1910 were too low. I therefore adopt the conclusion that Mr. Very's value at 2 calories should be reduced to about 1.70 calories.[1] It also follows that Langley's value of 2.22 calories per sq. cm. per minute, found on Mount Whitney, if reduced to the Smithsonian scale of pyrheliometry would be about 1.92 calories per sq. cm. per minute, which would agree exactly with the mean of all the values obtained on Mount Wilson from 1905 to 1910.[2]

We may now consider the lowest point of Mr. Very's curve.[3] From *Annals of the Astrophysical Observatory*, **2**, 93, 94, I find that the highest observations which have ever been made in Washington,

[1] This conclusion is strengthened by the fact that the well-established measurements of K. Ångström on the peak of Teneriffe in 1895 and 1896 were all below 1.64 calories as stated by him. If 3.9 per cent is added, they also reach 1.70 calories.

[2] "Report on the Mount Whitney Expedition," *Professional Papers Signal Service*, No. 15, p. 148, Table 120, columns 3, 4 and 5: also *Astrophysical Journal*, **33**, 194, 1911.

[3] *Astrophysical Journal*, **37**, 29, 1913.

at sea-level, of the radiation of the sun, when reduced to the scale of Smithsonian Revised Pyrheliometry of 1913 are as follows:

DETERMINATIONS OF SOLAR RADIATION AT WASHINGTON

Date	Hour Angle	Secant z	Pressure Aqueous Vapor	Calories per. Sq. cm. per Min.*
1902 Oct. 15	1^h8^m	1.552	.....	1.400
1904 Apr. 4	0 20	1.205	.....	1.310
Oct. 21	0 36	1.568	7.29mm.	1.447
1905 Mar. 2	1 22	1.557	.....	1.387
Sept. 26	0 33	1.328	4.57	1.365
1906 Feb. 15	1 3	1.689†	2.16	1.433
May 29	1 29	1.129	9.83	1.411
1907 Feb. 15	1 6	1.700	1.45	1.418

* According to Smithsonian Revised Pyrheliometry of 1913.

† The value given in *Annals* is misprinted.

From this I conclude that the lowest point of the curve which Mr. Very has shown is well taken at 1.5 cal. for sec. $z=1.4$.

Turning now to the point which he finds at 2.86 calories for a position in the free atmosphere at an elevation of 13,700 meters, I think this is subject to strong criticism.

The circumstances of the experiment were these: A ball of blackened copper supported by a balloon of above 2 m. diameter was alternately shaded by a double screen of aluminium and exposed to sunlight. At an elevation stated 13,700 m., barometric pressure stated 118 mm., air temperature stated 208° absolute Centigrade, the copper bulb in sunlight was stated 261° absolute Centigrade. Let r be the radius of the copper ball; a_1, a_2, a_3 its average absorption coefficients for solar rays, for its own proper emission, and for the radiation of the surroundings, respectively; S the intensity of solar radiation; σ a constant adapted to fit the radiation of the ball to Stefan's formula; K the coefficient of convection; Q the average intensity of radiation from surroundings, all per sq. cm. per min.; T_1 and T_2 the average temperatures of the ball and surroundings, respectively.

$$\begin{aligned} \text{Then solar radiation absorbed} &= \pi r^2 S a_1 \\ \text{Other radiation absorbed} &= 4\pi r^2 Q a_3 \\ \text{Radiation emitted} &= 4\pi r^2 \sigma T_1^4 a_2 \\ \text{Convection loss} &= 4\pi r^2 K(T_1 - T_2) \end{aligned}$$

Hence

$$\pi r^2 S a_1 + 4\pi r^2 Q a_3 = 4\pi r^2 \sigma T_1^4 a_2 + 4\pi r^2 K(T_1 - T_2)$$

Or

$$S = \frac{4\sigma T_1^4 a_2 + 4K(T_1 - T_2) - 4Q a_3}{a_1}$$

If the value a_1 is omitted, we shall not probably go 5 per cent astray. The values a_2 and a_3 are decidedly more uncertain, as indicated below under σ. T_1 and T_2 were observed. We may justly ask, however, whether a single determination of them by necessarily unsupervised registering apparatus in trying circumstances is entitled to great weight.

The remainder of the quantities are all unknown.

As for σ, the determinations of constants of emission in Stefan's formula appropriate for use with the actual blackened surfaces ordinarily employed are now covering the range from 0.50 to 1.40×10^{-12} cal. per sq. cm. per second.[1] The range appears to depend on very obscure details of surfaces of the bodies investigated.

As for Q, we are not informed as to the temperatures in sunlight, radiation coefficients, solar radiation reflected, or solid angles subtended, by the balloon, registering apparatus, aluminium screen, and layers of atmosphere sending radiation to the copper ball.

As for K, no experiments in conditions comparable to these are known to me. Certainly none such are given with sufficient detail for critical study in Mr. Very's communications.

From the paper of P. Compan just cited I find from a direct experiment that a blackened copper ball of 2 cm. diameter in free air cooled at the rate of 0.00789 calories per sq. cm. per min. per 1° C. temperature excess, when the air temperature was 5° C. and the ball temperature 50°.96 C. This cooling occurred at atmospheric pressure, and included all elements of cooling, air convection, air conduction, radiation, etc. If the copper ball referred to by Very cooled similarly we should have as the measure of solar radiation

$$4 \times 53 \times 0.00789 = 1.67 \text{ cal. per sq. cm. per min.}$$

The matter is, however, wholly hypothetical. So far as I can see,

[1] See P. Compan, *Annales de chim. et de phys.*, 7 sér., **26**, 571, 1902; H. Kayser, *Spectroscopie*, **2**, 78–80 and 133; C. Fery, *Soc. franc. de phys.*, Nr. 293, pp. 2–3, 1909.

there is no way of knowing the intensity of the solar radiation to within 100 per cent of itself from the experiment cited by Mr. Very. It is quite significant, I believe, that M. Violle himself has not indicated any numerical value as the result of the experiment.

I submit that a curve similar to Mr. Very's drawn through a point at 1.5 calories at sea-level, 1.7 calories at Mount Whitney level, and a wholly unknown value at 13,700 meters level may very likely be in excellent agreement with the value of 1.92 cal. per sq. cm. per min. at the outer limit of the atmosphere; and I shall continue to think so until something equal in weight to the 900 spectro-bolometric determinations already made indicates otherwise.

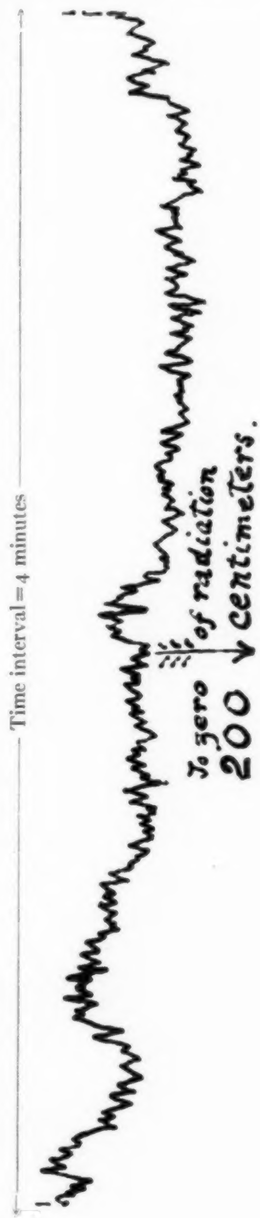


Fig. 1

II. ATMOSPHERIC TRANSMISSION

It may be freely admitted that for many stations at nearly all times, and for all stations at some times, the deeply indented actinograph curve shown by Mr. Very[1] is typical. But, as I shall show, for good stations at almost all times, and for ordinary stations some times, the case is altogether different.

Fig. 1 shows on the original scale a portion of the curve made on Mount Wilson on September 12, 1905, for the express purpose of testing the steadiness of the transmission of the atmosphere. The following notes were made at the time.

[1] *Astrophysical Journal*, **37**, 33, 1913.

Test of steadiness of sunlight.—A direct beam of sunlight was passed through the slit and both diaphragms of the spectro-bolometer, and reflected by the collimating mirror directly to the image forming mirror [without passing through the prism] and thence upon the bolometer. The beam was left out of focus so that it came from all parts of the sun. A diaphragm was provided over the image forming mirror, so as to reduce the equivalent deflection of the galvanometer to about 2 meters (200 centimeters). The spot of light reflected from the galvanometer was adjusted to fall near the middle of the recording photographic plate. The run was started at $2^h 27^m 25^s$ in good blue sky with a distant cloud gathering from the southwest. The run continued until $2^h 57^m$ when the clouds reached the sun.

From this quotation it will be seen that the sky at this time was probably by no means exceptionally good, but rather perhaps on account of the cloud we should regard it as decidedly below the average found upon Mount Wilson. Furthermore, the afternoon sky there hardly equals that of morning. The time of single swing of the galvanometer during these experiments was about 1.5 seconds and the bolometer was so fine that it responded more quickly to changes of radiation than the galvanometer could record. Under these circumstances the plate shows that *during the first 23 minutes of the run there was no change of radiation, either of short period or long, as great as 1 per cent.* The latter part of the curve shows a gradual falling-off of the radiation, due, I suppose, to the close approach of the cloud which at the end covered the sun.

The Smithsonian Astrophysical Observatory has in its possession several thousand automatic records, some of the spectro-bolometer, others of the recording water-flow pyrheliometer and other instruments, which show that the curve I have just referred to is by no means exceptional in showing an almost perfect uniformity from minute to minute in the transmission of the atmosphere not only above Mount Wilson but at Mount Whitney, Bassour, Algeria, and occasionally even at Washington. We have also several thousand pyrheliometric determinations of solar radiation made at Mount Wilson and elsewhere which indicate that on a great majority of the days observed the march of the intensity of solar radiation was perfectly regular and normal from a zenith distance of 70 or 75 degrees up to zenith distances of 20 or 30 degrees. Such a curve may be found in the writer's book on *The Sun*,[1] pp. 285 and 287.

[1] D. Appleton & Co., 1911.

The observations there referred to were begun as soon as the sun peeped above the horizon and were continued until 5 minutes before noon. Each sun exposure was 100 seconds. It is only the very last observation for the series which shows any falling-off of the intensity of solar radiation due to the approach of the haze or water-vapor which Mr. Very has described; and the falling-off which then appeared is only about 2 per cent of the highest value.

My colleague Mr. Fowle has kindly allowed me to take in advance of publication the data which are given below in regard to the quantity of water-vapor prevailing between Mount Wilson and the outer limit of the atmosphere and the changes of it which occur during the time interval covered by the ordinary bolographic observations.

During the observations of 1910 and 1911 on Mount Wilson, the mean quantity of precipitable water in the atmosphere between the station and the zenith was 6.9 mm. The number of days when the change of this quantity during the observations reached certain limits is as given in the following table:

CHANGE IN ATMOSPHERIC VAPOR DURING DAY OBSERVATIONS

Range	0 to 0.05 cm.	0.05 cm. to 0.10 cm.	0.10 cm. to 0.15 cm.	0.15 cm. to 0.20 cm.	0.20 cm. to 0.30 cm.	Above 0.30 cm.
1910	22 days	19 days	34 days	8 days	12 days	4 days
1911	14 days	21 days	21 days	8 days	9 days	7 days
Total	36 days	40 days	55 days	16 days	21 days	11 days

Average change, 1910, 0.13 cm.: 1911, 0.14 cm.;
Average increase, 1910, 0.11 cm.; 1911, 0.12 cm.;

From this it will appear that on a very great number of days of observation on Mount Wilson there has been no appreciable change at all in the quantity of water-vapor intervening between the observer and the sun. Further evidence will appear in the next section.

III. BOUGUER'S FORMULA

Mr. Very says:[1]

If it were possible to measure strictly homogeneous radiations, and if occasions could be found when the atmospheric quality remains unchanged between

[1] *Astrophysical Journal*, **37**, 32, 1913.

high-sun and low-sun observations, we could apply Bouguer's equation

$$R = Ap^{\epsilon}$$

in which R = solar radiation received normally on the unit of area at the surface of the earth, A = the solar constant, p = the coefficient of transmission, assumed to remain unchanged during the day, and ϵ = the air mass taken as unity for sea-level and normal pressure, so that, if B is the observed barometer reading at the place of observation and ζ the sun's zenith distance, $\epsilon = (B/760)$ sec. ζ, until zenith distances greater than 56° are reached, beyond which a small correction is required.

There seems to be so much confusion in regard to these matters that I shall quote here from pp. 14 and 15 of the *Annals of the Astrophysical Observatory of the Smithsonian Institution*, **2**.

Langley showed that the exponential formula would still approximately apply if we assume that the atmosphere is composed of thin layers concentric with the earth, each one of uniform transparency [during the two or three hours necessary to make a determination] but each differing slightly from the next adjacent one in transparency. This may be proved as follows:

PROOF OF FORMULA FOR TRANSMISSION

Imagine the atmosphere to be made up of n concentric layers so chosen in thickness as to produce separately equal barometric pressures, and let the number n be so great that the transparency of any single layer is sensibly uniform, although the layers may differ from each other in transparency by any gradual progression. The index of refraction of air is so near unity that there will be no sensible regular reflection in passing from one layer to the next, and the transmission of each layer may be expressed exponentially by Bouguer's formula, but with different coefficients of transmission for the several layers.

Thus, suppose E_0 to be the original intensity of a beam of light incident upon the outermost layer at the angle whose secant is m.

Then after passing successive layers the remaining intensities become

$$E_1 = E_0 a_1^{m_1}, \qquad E_2 = E_0 a_1^{m_1} \cdot a_2^{m_2}, \qquad E_n = E_0 a_1^{m_1} a_2^{m_2} \; . \; . \; . \; . \; a_n^{m_n}. \tag{1}$$

The value of the secant of the angle of incidence will change slightly in passing from layer to layer from two causes: first, the curvature of the earth; second, the refraction of the beam in air. These causes produce opposite effects, the first tending to increase the angle of incidence, the second tending to diminish it as the beam approaches the earth's surface. Their combined effect is dependent on the height to which the temperature exercises absorption and on the distribution of density with the height. But it is generally supposed that the absorption of the air above 40 miles from the earth's surface is negligible, and, remembering that the atmospheric density diminishes with the height, it appears that for zenith distances less than 70° the effect of change of the secant of the angle of the incident beam from the outermost

to the innermost layer of the atmosphere will not introduce error greater than 1 per cent. Accordingly for zenith distances less than 70° we may write approximately

$$E_n = E_0(a_1 a_2 \ \ldots \ldots \ a_n)^m \tag{2}$$

The symbols $a_1, a_2 \ldots \ldots a_n$ denote constants (providing no change of transparency occurs during the interval of time in question), and their values are slightly less than unity. We may substitute for their product a single constant, a, itself a proper fraction, and remembering that E_n is the intensity at the earth's surface, above denoted simply by E, we have

$$E = E_0 a^m \tag{3}$$

LIMITATIONS OF FORMULA

No mention is made in this expression of the barometric pressure, but it is easy to see that an alteration of barometric pressure would signify, under the conventions adopted in deriving the formula, a change in the number of layers, n. This would cause an alteration of the quantity a, which is the continued product of the transmission coefficients of the layers, by introducing additional multipliers $a_{n+1}, a_{n+2} \ldots \ldots$ or by the withdrawal of some $a_{n-1}, a_{n-2} \ldots \ldots$ Since we have no means of determining the value of the terms so introduced or taken away, there is no means of correcting for change of barometer in the use of the expression (3) and it would, for instance, be impossible to compute, from knowledge of the values of E, E_0, a, and m for one station, what would be the value of E at some station of different barometric pressure.[1]

From this we see that the unit of air mass to be taken for each station is the air mass traversed by beams from zenith celestial objects *between the station itself and the outer limit of the atmosphere*, and that the definition of unit air mass as that lying between sea-level and the outer limit of the atmosphere, as stated by Mr. Very, would be wholly indefensible unless the optical quality of the atmosphere was uniform from sea-level to its outer limit, which Mr. Very explicitly denies.

But he is not alone in his misconception of the applications of Bouguer's formula to the earth's atmosphere. Professor F. H. Bigelow is of the opinion that the unit of air mass is entirely indeterminate.[2] Professor Bigelow is also of the opinion that there is situated in the higher atmosphere a reflecting layer to which Bouguer's formula does not apply and which according to him

[1] This formula holds only for homogeneous rays.

[2] *Bericht über die erste Tagung der Strahlungskommission des Internationalen Meteorologischen Komites*, September 2 and 3, 1912, p. 17.

doubles the value of the solar constant. I would freely admit that if there were up there a sheet of plain parallel glass, or of water, its reflection would not follow Bouguer's formula, but rather the formula of Fresnel for the reflection of light.[1] But this is of course not the case. Any reflecting layer which exists in the atmosphere, so far as is known, consists of small particles distributed through a great thickness. To such a combination the formula of Bouguer does apply.

If there were any doubt about the applicability of Bouguer's formula to such a layer, it would have been removed during the past summer. For the volcano of Mt. Katmai in Alaska on June 6, 1912, sent up such a cloud of particles of volcanic glass and other ejecta to enormous heights that the atmosphere of the whole earth was made hazy by it for months. This haze was so great in quantity as to reduce the intensity of the direct solar beam by nearly or quite 20 per cent, yet the Bouguer transmission coefficients for homogeneous rays determined by spectro-bolometric observations at Mount Wilson and in Algeria took care so exactly of this floating obstruction that the average values of the solar constants obtained during the year 1912 agreed within 1 per cent of those obtained in former years.

We have at the Astrophysical Observatory several hundred days of observations from Mount Wilson, Mount Whitney, and Bassour, Algeria, for which the departures of the bolographic observations at individual wave-lengths are no greater than those recorded at p. 66 of the *Annals of the Astrophysical Observatory*, **2**. It will be noted that in this example of the applicability of Bouguer's formula the air masses varied from nearly 5 to about 1.2, or between zenith distances of 78° and 30°. Mr. Very, however, states that the proper part of the curve to compute from lies between 75° and 85° of zenith distance (air masses four to ten). This is not the fact, for the derivation of the exponential formula which I have given above shows distinctly that the formula is applicable to the case of the atmosphere as a whole only when the increase of path by obliquity of the beam in each of the layers of which the atmosphere

[1] See E. C. Pickering on "Applications of Fresnel's Formula for the Reflection of Light," *Proc. American Academy of Arts and Sciences*, 1873.

may in imagination be composed is approximately the same. This is no longer true above 70° of zenith distance, although the departure is not very serious up to 75°; but beyond this zenith distance the departures from proportionality in the path within the different layers of the atmosphere, due to curvature and refraction, soon become very great.[1] In these circumstances Bouguer's formula no longer applies. It is for this reason that in our work we do not observe ordinarily at greater zenith distances than 75°.

Mr. Very has another criticism, for he says "this of course is not true except for absolutely homogeneous rays which are never measured by the spectro-bolometer."

Referring to Volume **2** of the *Annals of the Astrophysical Observatory*, p. 16, I quote:

Suppose a ray composed of amounts A_0 and B_0 of light of two different wave-lengths to pass through a homogeneous stratum of air, and let a and b denote the fractions of the respective kinds of light transmitted by the stratum at vertical incidence. Suppose the intensity of the beam after transmission to be observed, first when the secant of the angle of incidence is m, and again when the secant is $2m$. Let C_1 and C_2 represent the results of these observations; let c be the coefficient of vertical transmission which they yield and C_0 the intensity of the beam before transmission, as computed from the observed data.

By Bouguer's formula:

$$C_1 = C_0 c^m \text{ and } C_2 = C_0 c^{2m}$$

Hence

$$c^m = \frac{C_2}{C_1} \text{ and } C_0 = \frac{C_1^2}{C_2}$$

But the original intensity of the beam was in reality $A_0 + B_0$; its intensity observed as $C_1 = A_0 a^m + B_0 b^m$ and its intensity observed as $C_2 = A_0 a^{2m} + B_0 b^{2m}$. If there is a difference between the real and computed intensity prior to transmission, this is $A_0 + B_0 - C_0$, and substituting for C_0 we have the defect of C_0 as follows:

$$A_0 + B_0 - C_0 = A_0 + B_0 - \frac{(A_0 a^m + B_0 b^m)^2}{A_0 a^{2m} + B_0 b^{2m}} = \frac{A_0 B_0}{A_0 a^{2m} + B_0 b^{2m}} (a^m - b^m)^2 .$$

Suppose, better to fix our ideas, that we introduce numerical values into this expression, in order to see how great the error which Mr. Very implies may really be. Let $A_0 = B_0 = 0.5$, let $m = 1$, let $a = 0.9$, and let $b = 0.5$, then we find $A_0 + B_0 - C_0 = 0.093$.

Hence it appears that if the bolometer habitually fails to

[1] See *Annals Astrophysical Observatory*, **2**, 63, 64.

recognize differences of transmission between adjacent wave-lengths amounting to the difference between 0.5 and 0.9, the correction to be applied to the results on account of this failure on its part will amount to a little less than 10 per cent. Mr. Very maintains that the nature of that which diminishes the intensity of the solar beam in passing through the atmosphere is such that lines of almost complete absorption and lines of almost complete transmission habitually succeed one another so intimately that no spectroscope has ever been able to discover these hypothetical absorption lines. It is much more likely that the atmosphere in producing what is loosely termed its "general absorption" exercises a scattering which is a continuous function of the wave-length, as supposed by Lord Rayleigh, rather than that it presents a curious absorption phenomena like that indicated by Mr. Very. If so, the lack of homogeneity in the rays investigated by the bolometer will make a defect in our solar constant observations which is wholly negligible.

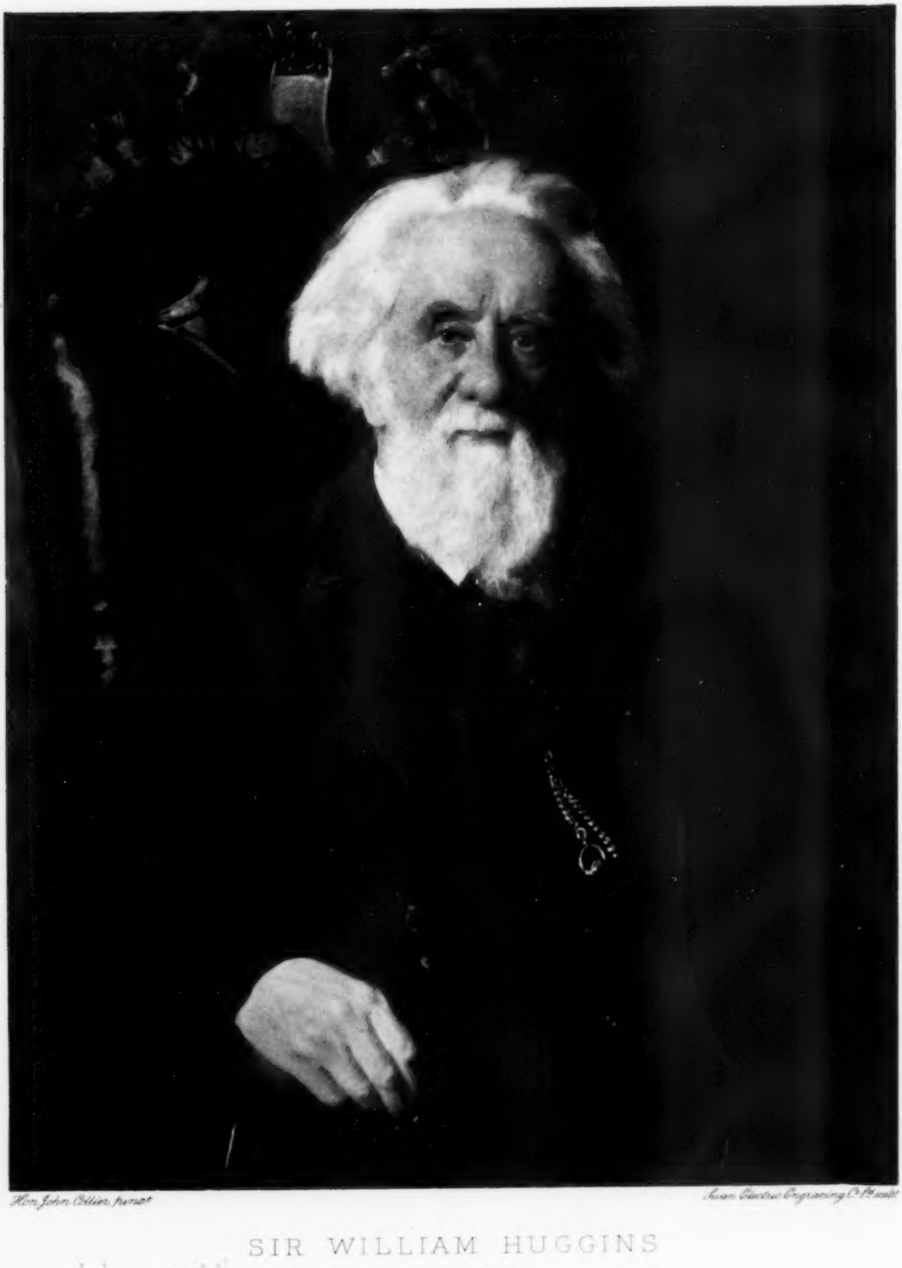
CONCLUSION

Mr. Very presents evidence to show that pyrheliometric measurements have been made within the atmosphere exceeding our value of the "solar constant." I have controverted this evidence.

He has admitted that our method of determining the "solar constant" would be sound if the atmosphere remained of uniform transparency while we observe, and if the bolometer could measure homogeneous rays, but he denies that these conditions are well enough fulfilled. I have shown how probable it is that they are.

Mr. Very finds that at least one day has occurred perfect enough to suit him. It seems remarkable that out of 900 days (some yielding sea-level values equal to that quoted by Mr. Very) in which we have observed with the spectro-bolometer, not any approached so near this paragon condition *as to appreciably raise our "solar constant" values*, computed, as Mr. Very admits, by the right method, and lacking only good sky and homogeneous rays for success.

Smithsonian Astrophysical Observatory
Washington
February 3, 1913



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K.C.B., F.R.S.

Born 1824 Died 19[illegible]